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FOREWORD

Some of the most exciting adventures of modern times occur in the field of physics. They may profoundly alter our whole view of the nature of things and will in all probability lead to far-reaching changes in practical life. It is very difficult for any but experts to understand what is going on, not, as so many non-scientific people are fond of asserting, because men of science are incapable of expressing themselves clearly or are unwilling to try, but for the simple reason that many aspects of the subject are of very great inherent difficulty. They are based upon unfamiliar conceptions, often developed by advanced mathematical reasoning and sometimes expressed in a complex and abstract symbolism. Quite apart from this the sheer magnitude of the material is daunting, and the situation is aggravated by the multiplicity of journals in which the original contributions are published.

It is clear, therefore, that a very valuable purpose can be served by a journal in which experts will endeavour to present the whole field in perspective to a wider circle of their scientific colleagues.

Many useful things can be done. General reviews can be made, the history of important developments can be traced, the philosophical foundations of current ideas can be discussed and the general consequences of important discoveries assessed.

A successful journal conceived in this manner can do much to help science take its rightful place in the world today.

C. N. HINSHELWOOD.

Molecular beams

by O. R. FRISCH
Cavendish Laboratory, Cambridge

SUMMARY

Molecular beams are beams of molecules (which may be single atoms) travelling through vacuum at thermal velocities. They have been used to explore many properties of atoms and molecules, chiefly through their deflection in magnetic and electric inhomogeneous fields. Atomic beams first clearly established the phenomenon of space quantization, which means that the magnetic (or electric) energy of an atomic system in a given field can have only certain discrete values, and is not a continuum as classical physics supposed. Transitions between those energy states can be induced by radio-frequency fields; sharp resonances are observed and can be used to get accurate values of atomic and nuclear constants or to provide accurate frequency standards (' atomic clocks '). Molecular beams have also been used to test de Broglie's surmise that beams of particles should show wave properties (diffraction), to study collisions between molecules, and to supply spectroscopic sources in which the line broadening by Doppler effect is greatly reduced.

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1. INTRODUCTION

Much of what we know about atomic structure has been found through the study of beams of fast-moving particles. By the study of cathode rays, the electron and the X-rays were discovered. Beams of positive ions revealed the existence of isotopes. The pursuit of nuclear physics is based on the use of fast particles, natural or artificially accelerated. All those techniques have spread to many laboratories all over the world.

By contrast, the study of molecular beams, i.e. beams of atoms or molecules flying through vacuum at thermal speed—a mile a second or so, has been almost a family affair. At atmospheric pressure a molecule will travel only some 10^{-6} cm before colliding with another molecule. Hence a pressure well below 0·001 mm mercury has to be achieved before this mean free path reaches the order of inches, and such a good vacuum was not easily available before the first fast pumps were introduced by Gaede in Germany about 1910. So the study of molecular beams started at a time when their more robust brothers, the electron and ion beams, were already well established. It was Otto Stern who first showed the usefulness and versatility of the molecular-beam method, and most of the work has been done by his direct and indirect pupils.

The first to demonstrate the travel of molecules in a straight line through a vacuum was a Frenchman, Dunoyer (1911). He heated sodium in one end of an evacuated glass tube which had two constrictions, dividing it into three compartments (rather like a string of three sausages). In a good vacuum a sharply defined deposit of sodium was formed on the cooled glass wall at the far end, covering just that area which could be reached by sodium atoms travelling in a straight line through the two constrictions. The deposit became blurred when the vacuum was poor. So Dunoyer had observed the first (rather crude) molecular beam and also its scattering in a residual gas.

After the first war, molecular-beam studies were taken up at Frankfurt am Main. Max Born and Miss Bormann observed how the mean free path of silver atoms varied with gas pressure; and Stern (1920) published a rough measurement of the mean velocity of the same atoms, obtained by observing the deflection of the beam by the Coriolis force when the whole arrangement was rotated at 1500 rev./min. Soon afterwards he saw that an important question of quantum physics might be settled by studying the deflection of a beam of silver atoms in an inhomogeneous magnetic field. Certain spectroscopic facts had suggested that every silver atom should behave like a magnetic spinning top, but with the startling proviso that in a magnetic field—however weak!—the magnetic moment of the atom would always be lined up either in the direction of the field or opposite to it. This predicted 'space quantization' was in striking contrast to classical physics according to which the magnetic moment might with equal probability point in any direction, and many people thought it was merely a formal device for computing spectra. Gerlach had measured how inhomogeneous the magnetic field was near a pole piece with a sharp edge, and Stern estimated that silver atoms, travelling along such an edge, would be significantly deflected, by an amount depending on their orientation. The experiment (Gerlach and Stern 1922) showed that space quantization was real: the beam was split in two and not merely broadened as it would have been, had the atoms been oriented at random.

Before further advances could be made a good deal of technical development was needed. In those early experiments the beam was detected simply by placing a glass or metal plate in its way and waiting for a visible deposit to form. This method was slow and inaccurate, even though intensities could be measured roughly from the appearance time; and it was limited to materials that would condense readily. As soon as Stern had settled down in Hamburg (1924) he started to develop the technique systematically. Those thirty 'Untersuchungen zur Molekularstrahlmethode' which were published from his laboratory between 1926 and 1933 (when Hitler came to power and Stern went to the U.S.A.) are a monument of foresight, planning and achievement. In the first of those papers (Stern 1926) much of the subsequent work there and elsewhere was foreshadowed, and the basic conditions for effective work with molecular beams were laid down with classic lucidity.

2. Production and detection of molecular beams

The beam emerges through an aperture—usually a narrow slit—from a container which is called the oven, even though it has to be heated only if the material otherwise has not enough vapour pressure or if one wishes to dissociate its molecules. The emerging molecules fan out in all directions and most of them strike the wall of the vacuum container and are either condensed or removed by pumping. Only a few pass through a second slit, the collimator, into another compartment; they form the molecular beam which one wishes to study.

The pressure in the oven must be low enough so that the molecules emerge without jostling; that means, the mean free path must be greater than the width of the oven slit. A narrower slit allows the use of higher oven pressure; hence the number of molecules emerging per centimetre slit length remains the same (around 10^{18}/sec), and slits as narrow as 0·01 mm are common. Knife-edge jaws allow the molecules to emerge at all angles, and only a small fraction will pass through the collimating slit to form the beam. A better polar diagram is obtained with a 'channel slit' having flat-edged jaws. It is true that the beam intensity is lowered by such a slit; but the total escape rate from the oven is lowered even more, which means economy in beam material and greater ease in maintaining a good vacuum. For that latter reason, an extra slit (or even two) between oven and collimator may be used to prevent most of the unwanted molecules from entering the main part of the apparatus.

The problem of measuring beams of non-condensing molecules was solved by Knauer and Stern (1929). The beam is faced by a container with a small opening through which the beam molecules enter and accumulate until as many escape as enter during a given time-interval. A pressure proportional to the beam intensity is thus built up which is measured by a manometer, usually of the hot wire (Pirani) type. Pressure fluctuations can be eliminated by forming a Wheatstone bridge of two such manometers, one with an opening not facing the beam. In this way one can measure pressure differences of 10^{-8} mm or less, a thousand times smaller than the pressure built up by a typical direct beam. Here again a channel slit is useful because it reduces the rate of escape but not—if properly aligned—the rate of entry. The design of those manometers requires great care; in particular, the volume should be less than one c.c. so as to keep the equilibrium time down to a few seconds (see, e.g., Kellogg *et al.* 1939).

Another important detection method was first demonstrated by Taylor (1929) and is based on the fact that atoms or molecules with low ionization energy, on striking a suitable hot metal surface, may be evaporated from it as positive ions. Thus if a glowing tungsten or platinum wire is placed in the beam, a nearby negative electrode will collect the evaporating ions, and the current is usually big enough to be easy to measure. In this way one can detect some alkali atoms and many of their compounds as well as some other atoms and molecules; the range can be further extended by slightly oxidizing the wire and thereby raising its work function from 4·5 v to about 6 v. In the same way, halogen atoms and compounds can be detected because they have a slight tendency of being evaporated in the form of *negative* ions.

Instead of just collecting the ions one can make them pass through a mass spectrometer and select one of several isotopes; furthermore, by letting the emerging ions fall on an electron multiplier, one can count them one by one and thus achieve the maximum possible sensitivity.

There is still a considerable variety of atoms and molecules that can be measured neither by a manometer nor by a hot wire, nor any other of various methods that work for particular species. One can ionize them in mid-flight by bombarding the beam with an intense stream of electrons (Wessel and Lew 1953), but it seems difficult to ionize more than a fraction of one per cent of the beam molecules.

During the last decade the study of short-lived radioactive isotopes has become very popular. Here the measurement is very easy: the beam is simply allowed to condense for a certain time on a cooled target which is then removed through an airlock and placed near a Geiger or scintillation counter. In this way a specific isotope (identified, e.g., by its half-life) can be measured in the presence of others and of a large excess of stable, inactive isotopes.

3. Deflecting fields

A magnetic dipole placed in a magnetic field has a potential energy $E_{\mathrm{m}} = -\boldsymbol{\mu}\cdot\mathbf{H}$ where $\boldsymbol{\mu}$ and $\mathbf{H}$ are vectors representing the magnetic moment and the field intensity. If the field is inhomogeneous there is a force acting on the dipole: $\mathbf{F} = -\operatorname{grad} E_{\mathrm{m}}$. This will cause deflection of a molecular beam if $\mathbf{F}$ has a component normal to the beam direction.

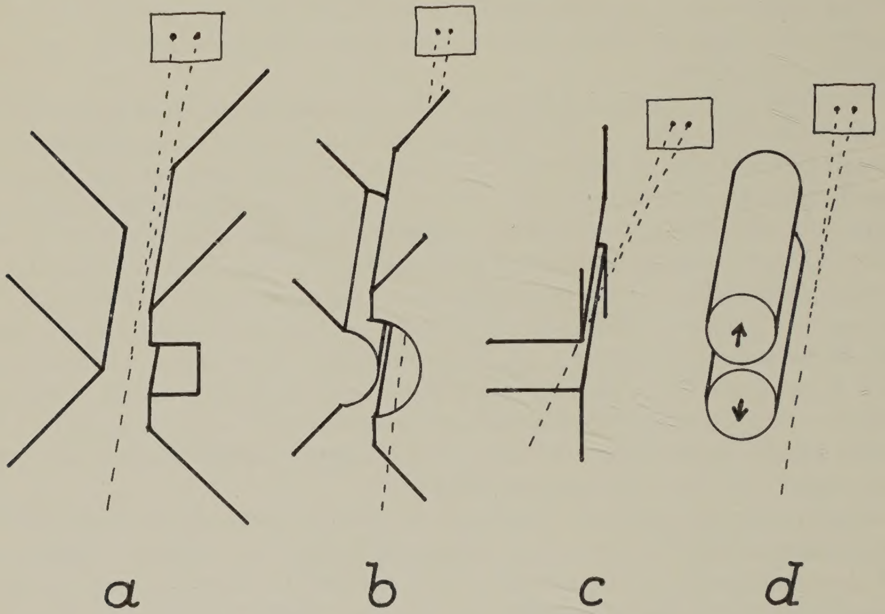


Fig. 1. Types of inhomogeneous fields; (*a*) is the Stern–Gerlach field; (*b*) is a version in which the field can be computed (approximately) from the dimensions; (*c*) is the arrangement due to I. I. Rabi, where the deflection depends only on the angle of incidence and on the field values beween and outside the pole pieces; (*d*) is an arrangement of two cylindrical conductors through which a strong current is passed in opposite directions. In (*d*) the field is similar to that in (*b*) but can be accurately computed because no iron is used.

Stern and Gerlach used the field close to a sharp-edged pole piece, the other pole piece being flat at first, later containing a groove (fig. 1 *a*). A smaller but more uniform and more nearly calculable inhomogeneity can be obtained with pole pieces having suitable circular cross sections (fig. 1 *b*). In these configurations the gradient is in the same direction as the magnetic field, but that is not necessary; for instance one can use the edge portion of the homogeneous field between flat pole pieces. Rabi (1929) showed that the deflection of a beam passing obliquely through such a region (fig. 1 *c*) depended only on the angle between the beam and the edge and on the energies of the dipole inside and outside the field; there is thus no need for the difficult measurement of the field in the inhomogeneous region. For this, however, one pays by getting rather smaller deflections. Fields without iron can be calculated from the configuration of the electric currents used; a convenient arrangement (fig. 1 *d*) is that of two parallel, adjacent cylinders through which the same strong current is passed in opposite directions, the field being similar to that in fig. 1 (*b*).

For a long time the aim in designing fields was to get an inhomogeneity which was strong and yet reasonably uniform over the cross section of the beam. More recently it has been realized that fields can be designed to have a focusing effect. For instance, a field produced by six electric currents, flowing in alternate directions along the edges of a regular hexagonal prism, will produce a field intensity proportional to the square of the distance from the axis of the prism; magnetic atoms with the appropriate orientation ($E_{\mathrm{m}} > 0$) will experience a quasi-elastic force towards this axis (Friedburg and Paul 1951). Such a field (which is a special kind of octupole field) can also be produced by a magnet with six pole pieces. If μ is proportional to H (see § 4) then one requires a field intensity proportional to the *first* power of the distance from the axis; in this case one uses a quadrupole field (Bennewitz and Paul 1954) rather than one with six poles. The focal length of the field depends of course on the speed of the atoms (or molecules) as well as on their orientation, and such a field can therefore be used to select atoms of a small velocity range and of a given orientation.

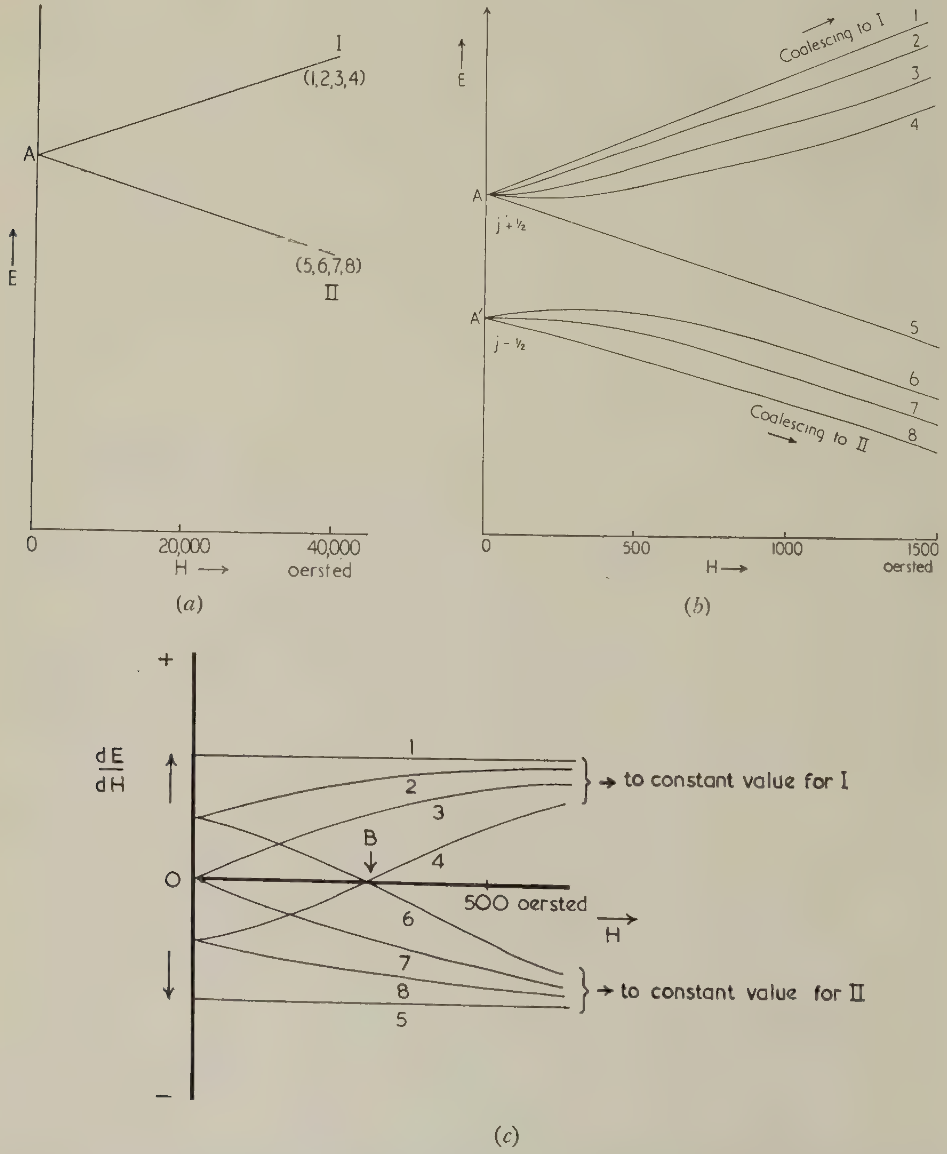
An atom never has an electric dipole moment. But molecules often do, and can then be deflected by inhomogeneous electric fields. Here again quadrupole fields with their useful focusing properties are often used (see e.g., Russell 1958).

4. Nuclear magnetic moments

The Stern–Gerlach experiment was soon followed by successful and increasingly accurate measurements of the magnetic moments of alkali atoms and others. About that same time Pauli, in order to explain the hyperfine structure of spectral lines, suggested that atomic nuclei might have small magnetic moments, and Stern considered the possibility of measuring those moments by the molecular-beam method. They were expected to be a thousand times smaller than the atomic moments, and so the technique had to be refined very considerably.

The first nucleus requiring to be studied was clearly the proton. It was expected to obey Dirac's theory and thus to have the same spin $\frac{1}{2}$ (in the usual units of $h/2\pi$) as the electron but a magnetic moment 1836 times smaller than that of the electron (since it is 1836 times heavier). Stern used beams of hydrogen, and it was largely for this purpose that he developed manometric detection. Hydrogen molecules are of two kinds: in *parahydrogen* the spins of the two protons are anti-parallel so that the magnetic moments cancel, while in *orthohydrogen* they are parallel, giving a momentum of 1 and twice the proton's magnetic moment. Because of Pauli's exclusion principle, the rotation of the molecule is limited to odd quantum numbers for orthohydrogen, to even ones for parahydrogen. At room temperature (or higher), the composition of hydrogen is one-quarter parahydrogen, and three-quarters orthohydrogen; by cooling in the presence of a catalyst (e.g. charcoal) it can all be converted into parahydrogen.

It was first checked that cold parahydrogen (the 'oven' could be cooled with liquid air) went through the inhomogeneous field without deflection, having neither spin nor molecular rotation. Cold orthohydrogen, on the other hand, has one unit of rotation and one of nuclear spin. In a magnetic field an angular momentum j has $2j+1$ orientations; this makes three orientations each for the spin and the rotation of the orthohydrogen molecules. So the beam is split into nine components, each of which (except the undeflected middle one) is spread into a diffuse band by the Maxwellian velocity distribution of the molecules. Because of that spread, only three maxima were observed, and the effect of the proton spin had to be extracted by comparing curves obtained at different temperatures and orthohydrogen contents. The magnetic moment of the proton turned out to be 2·5 times larger than expected, to everybody's surprise (Frisch and Stern 1933); later measurements by a resonance method raised that figure to 2·79274 ± 0·00006. This was the first sign that the proton does not obey Dirac's theory and is indeed a much more complex particle than the electron.



(a) (b)

(c)

Fig. 2. Here (a) shows the energy of a sodium atom in a strong magnetic field; the two lines I and II correspond to the two orientations of the electron spin while the nuclear spin has negligible effect; (b) shows the transition from the condition at very low field where electron and nuclear spin are coupled to the gradual decoupling at higher fields; (c) shows, for the same range of weak magnetic fields as in (b), the variation of dE/dH, the force on the sodium atom per unit field gradient, and the value (at B) of H for which two of the substates experience no force.

A different way of measuring nuclear moments was developed by Rabi and his school in New York, from 1931 onward. It depended on the interaction between the electron spin and the nuclear spin in the atom and was thus closely related to the spectroscopic study of hyperfine structure. For an example, let us look at the energy states of a sodium atom (^{23}Na) in a magnetic field. ^{23}Na has a nuclear spin $j=\frac{3}{2}$ which has four possible orientations in the field; but because of the smallness of the nuclear magnetic moment this has very little effect on the energy which is determined mainly by the orientation of the valency electron. So *on the scale of fig.* 2 (*a*) the pattern consists simply of two straight lines I and II corresponding to $E=E_0 \pm \mu_e \mathbf{H}$, where E_0 is the energy in zero field, represented *on this scale* by the single point A.

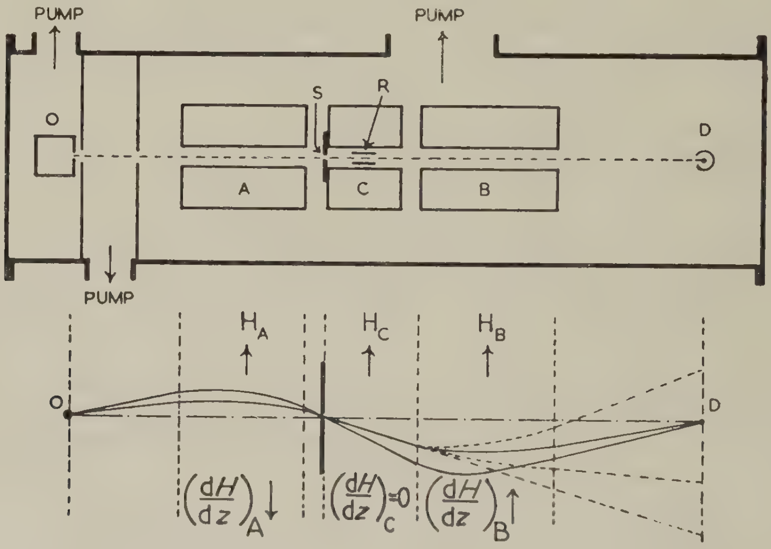
However this separate treatment of electron and nucleus is permissible only for strong fields, whereas in very weak fields the coupling between the two becomes the dominant feature. The two spins then combine to form a system with an angular momentum of either $j+\frac{1}{2}$ or $j-\frac{1}{2}$, and there are in fact two separate zero-field energies represented by A and A' (fig. 2 *b*). Each of these two states is split into $2j+2$ and $2j$ substates respectively by the external field. For ^{23}Na with $j=\frac{3}{2}$, there are five substates for the upper state and three for the lower one; these have been numbered consecutively, and in strong fields they coalesce (1, 2, 3, 4) to give I and (5, 6, 7, 8) to give II. At intermediate fields there is a smooth transition as fig. 2 (*b*) indicates between this pattern and that of fig. 2 (*a*) where electron and nucleus are decoupled by a strong field.

The deflection of a beam is governed by dE/dH (see § 3), and this quantity is plotted in fig. 2 (*c*). Substates 1, 2, 3, 4 tend towards the constant dE/dH of I, and 6, 7, 8, 5 towards that of II. This shows clearly that while a Stern–Gerlach experiment at low magnetic fields gives information on the nuclear spin, at high fields it does not. In particular it is seen that at certain field values (the *one* value for the case of ^{23}Na is at B) some of the atoms will remain undeflected, and a detector placed at the position of the undeflected beam will indicate sharp maxima at those field values; the number and position of those maxima tells one the nuclear spin j and the magnetic moment of the nucleus. Actually, to get the magnetic moment one has to compute the electronic wave function near the nucleus, and this cannot be done very accurately except for the hydrogen atom; but the small variation of the wave function from one isotope of an element to the other can be computed, and then the relative magnetic moment of isotopic nuclei can be obtained very accurately, to four or five decimals.

5. THE RESONANCE METHOD

Almost from the beginning, people had wondered what would happen if atoms passed through a region where the magnetic field rapidly changed direction. Would the atomic magnets follow those changes, however rapid? Stern suspected that they would not necessarily do so if the atoms experienced a significant change in field direction within one Larmor period, the time in which the atom would, classically speaking, precess once about the field. Experiments in his laboratory (Frisch and Segré 1933) showed that atoms could indeed be 'shaken off' and made to change their spin orientation, on passing through a weak enough field which changes direction fast enough.

It was a great and exciting advance when it was shown (Rabi *et al.* 1938) that such transitions between spin states could be induced in a homogeneous field by a superimposed radio-frequency field. The frequency ν at which resonance occurs indicates, through Planck's quantum condition $E_1 - E_2 = h\nu$, the energy difference between the two spin states. The only quantities that need be measured accurately are ν, and H the intensity of the homogeneous field in which the transitions are made to take place. Inhomogeneous fields are still used, but only as polarizers and analysers. The beam passes first through the polarizer (the A-field) where it is split in two beams, one of which is then eliminated by placing an obstacle in its way. The other beam then passes through a homogeneous field (C-field) which also contains a wire loop connected to a radio-frequency generator. Finally it passes through another inhomogeneous field (the B-field) which serves as analyser. Resonance is observed either as a decrease ('flop-out') or an increase ('flop-in') of the detector signal, depending on whether the detector has been placed in the position of the undisturbed beam or in that where the re-oriented atoms are expected. A 'flop-out' arrangement is shown schematically in fig. 3.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H_A
H_C
H_B
O
D
$\left(\frac{dH}{dz}\right)_A \downarrow$
$\left(\frac{dH}{dz}\right)_C = 0$
$\left(\frac{dH}{dz}\right)_B \uparrow$

Fig. 3. Above: schematic arrangement of a molecular beam apparatus for Rabi's resonance method. The beam emerging from the oven O passes through two foreslits and is finally collimated by the slit S; A and B are inhomogeneous deflecting fields with gradients dH/dz in opposite directions; C is a homogeneous field with $dH/dz = 0$; R is a coil or loop for producing a radio-frequency field.

Below: the full lines show the paths of molecules from O to the detector D in the absence of resonance, and the dotted lines what happens when there is resonance to the radio-frequency field and the molecules 'flop-out'. (Deflections are greatly exaggerated.)

The advantage is, in the first place, that it is no longer necessary to know accurately either the velocity distribution in the beam or the inhomogeneity of the deflecting field; the amount of deflection is no longer important. But there are more fundamental advantages. In the deflection method one can measure no more than dE/dH for the various magnetic states; one can, in principle, construct fig. 2 (*c*) from the measurements. With the resonance method, however, the energy differences between different magnetic states are measured, and thus fig. 2(*b*) can be constructed and not merely its derivative, fig. 2(*c*). Optical spectroscopy does the same, in principle; but the interpretation is much more complex because the hyperfine structure of an optical line reflects the hyperfine structures of the two electronic levels between which the optical transition takes place whereas the molecular beam explores the properties of one level. Also the accuracy is higher here because the energy differences between the magnetic states are measured directly whereas in optical spectroscopy they appear as minute differences between the very much larger wave-numbers of the optical line components.

Most of the molecular beam work in the last twenty years has been done with the resonance method. All the alkali-metal atoms—stable and unstable isotopes—have been explored as well as other atoms whose electronic angular momentum is greater than $\frac{1}{2}$ and where accordingly the energy pattern is more complex than the one shown in fig. 2. In addition, many atoms and molecules have been studied which do not possess an electronic moment; this necessitates much longer and narrower A and B fields, but the energy states of the nuclear moment in the C-field are not affected by an electron moment, and so one gets accurate values for the nuclear magnetic moment without having to know the electronic wave function.

An interesting variant (Hughes 1947) is the use of inhomogeneous *electric* fields for the study of molecules such as caesium iodide. Here the deflection rises at first quadratically with the field (because in the absence of a field the rotating dumb-bell has no static dipole moment) and depends strongly on the rotational quantum number. Hence it is possible to select molecules in one particular rotational state for study. Transitions showing sharp resonance can be caused by *electric* r.f. fields, superimposed on a constant electric or magnetic field or both. The possibilities are numerous; for a recent review see Schlier (1957).

An important rival to the molecular-beam method arose in 1946 when it was discovered that nuclear paramagnetic resonance could be observed in liquid or solid specimens by its effect on electric circuits ('nuclear induction'). This method was simpler and capable of very great accuracy; but it was limited to substances available at least by the milligramme. Furthermore the electric fields present in dense materials interact strongly with nuclear electric quadrupole moments; this gave rise to a lot of interesting crystal physics, but also to many difficulties (for a review, see Pound 1952). By the use of either this method or of molecular beams, values of nuclear magnetic dipole moments and electric quadrupole moments have been measured to five decimals or better, an accuracy that could never have been achieved with optical methods.

But optical spectroscopy has its advantages, too. The isotope shift, i.e. the difference in wavelength between corresponding lines of different isotopes, cannot be observed in any other way; and the molecular-beam method cannot easily deal with excited states of the usual short-lived kind. Metastable atoms, however, often live a millisecond or longer and can thus survive the journey along an atomic beam. This is a fruitful field of experimentation, which was opened by Lamb and Retherford (1947) in one of the most important experiments of recent times.

6. The lamb shift

The first excited state of the hydrogen atom has three fine-structure components; $s^{1/2}$, $p^{1/2}$ and $p^{3/2}$ in the usual spectroscopic notation. The purpose of the Lamb–Retherford experiment was to study those states by observing transitions between them. Since the ground state is an s-state the $s^{1/2}$ state is metastable (s–s transitions being forbidden) and can easily survive the time of travel in an atomic beam. If, however, radiation of the appropriate frequency (about 10 000 Mc/s) corresponding to the energy difference between the $s^{1/2}$ and $p^{3/2}$ states is present, transitions will be caused to the $p^{3/2}$-state, which is not metastable; the atoms will then quickly drop into the ground state so that fewer excited atoms will arrive at the detector. The detector is simply a metal plate from which excited atoms liberate electrons on striking it whereas atoms in the ground state do not.

The excited atoms are produced by first dissociating molecular hydrogen in a tungsten oven heated to 2500°K and then bombarding the emerging atoms with electrons of 11 ev energy. The beam, now containing a small number of atoms in the metastable $s^{1/2}$-state, passes through a cavity in which it can be subjected to a radio-frequency field. The whole arrangement is in a homogeneous static magnetic field which can be varied so that the Zeeman effect of the transitions can be studied.

The result was a great surprise. While the influence of the magnetic field was exactly as predicted, the absolute values of the resonance frequencies were not. Extrapolated to zero magnetic field, the energy difference between the $s^{1/2}$ and the $p^{3/2}$ states was about 1000 Mc/s less than predicted; furthermore, the difference between the $s^{1/2}$ and the $p^{1/2}$ state did not, as predicted, approach zero with diminishing magnetic field but approached a finite value of 1060 Mc/s. In other words, everything agreed with the computations from Dirac's theory except that the $s^{1/2}$ state was higher by 1060 Mc/s. This small difference has come to be known as the Lamb Shift. Visual spectroscopy, where it amounts to only 0·03 cm^{-1}, had given hints of such a shift; but it could never have measured it to one part in 10^4, which was done by Triebwasser *et al.* (1953).

The theoreticians rallied quickly from their surprise. Within a few weeks the next approximation of Dirac's theory had been computed, and further improvements in both theory and experiment have given agreement within 0·05 per cent. This success greatly strengthened the theoreticians' confidence in their methods of eliminating infinite results from their field theory calculations, and the award of the Nobel prize to Lamb in 1955 was a fitting reward for an experiment that released such a flood of important theoretical developments.

7. ATOMIC CLOCKS; THE MASER

The resonances observed with molecular beams are very sharp because they correspond to transitions in atoms in free space, undisturbed by collision, and the natural damping is extremely low. If the C-field is sufficiently uniform the line width may be as low as about half the reciprocal of the mean time which the atom spends in the radio-frequency field. Typically, that time is about a millisecond and the width a few hundred cycles per second; the centre of the resonance can then be located, with care, to a few cycles per second. The use of two short synchronous r.f. fields some distance apart (Ramsey 1949) instead of one extended field sharpens the resonance still more; and, what is more, the unavoidable inhomogeneity of the intervening magnetic field has much less effect. In this way a frequency standard can be realized which is constant to within a few cycles per second and the frequency of which depends only on the nature of the atoms used and not on the shape and size of a manufactured object such as a tuning fork or a quartz crystal.

For various reasons, caesium atoms are most commonly used for this, and the transition is that between the two hyperfine structure states of the (electronic) ground state. There is only one stable caesium isotope, with nuclear spin $\frac{7}{2}$; combining this in a parallel or anti-parallel orientation with the spin $\frac{1}{2}$ of the valency electron, we get 4 and 3 for the spins of the two states. In a magnetic field those states split into 9 and 7 components, respectively; however, one of the many possible transitions has a frequency which depends only quadratically on the magnetic field and is thus practically unaffected by a field of 0·1 oersted which at the same time causes the other transitions to have sufficiently different frequencies to eliminate confusion.

Instruments of that kind, called caesium clocks, are now made commercially. The molecular-beam apparatus is sealed off and works with such a weak caesium beam that one filling of the oven lasts for years. The signal from the detector is made to control the radio-frequency generator so that accurate resonance is automatically maintained, and circuits for counting the oscillations are included. A constancy of about one part in 10^{10} has been claimed, and the frequency of $9\,129\,631\,830 \pm 10$ c/s is probably the most accurately known physical quantity; the uncertainty is chiefly due to the difficulty of finding the length of the mean solar day ($= 86\,400$ sec) from astronomical observations.

The earlier ammonia clock is less accurate, but the principle on which it is based is particularly interesting and has led to important developments in electronics and solid-state physics. Gordon *et al.* (1954) showed that a beam of ammonia molecules could be used as a generator or amplifier of microwaves. A snapshot of an ammonia molecule would show, crudely speaking, the three hydrogen atoms in an equilateral triangle, with the nitrogen atom in a central position either above or below the plane of the triangle. The two positions are separated by a potential barrier which, in quantum theory, is not unsurmountable; the nitrogen atom can oscillate between the two positions with a frequency of 23 870 Mc/s. Neither position represents an eigenstate; for that, one has to form either a symmetrical or an unsymmetrical mixture of the wavefunctions describing the two positions. Those two eigenfunctions differ in energy by 10^{-4} ev, and a transition between them emits (or absorbs) the oscillation frequency just mentioned.

In the presence of an electric field the energy of the upper state is raised still further while that of the lower is lowered. Hence it is possible to separate the two states by an inhomogeneous electric field, and Gordon *et al.* (1954) did that very effectively by sending a rough beam of ammonia molecules through an electric quadrupole field which focused the molecules in the upper state while the others got dispersed. The beam then entered a cavity tuned to the inversion frequency, and it was found that a small oscillating electric field in the cavity caused stimulated emission of the inversion frequency from the molecules so that the oscillations got appreciably stronger; if the beam was strong enough it would even *excite* oscillations to a power of about 10^{-8} w (a sizeable part of the excitation energy carried by the beam) without any supply of electric power.

This device for Microwave Amplification by Stimulated Emission of Radiation was called MASER by the inventors. It is a remarkably simple frequency standard, but not an extremely accurate one; the inversion frequency is split into a number of components by the effects of nuclear spin and molecular rotation, and the frequency of the oscillator depends slightly on the dimensions of the cavity. But as an amplifier the MASER is rapidly gaining ground, in particular since it was discovered that the essential requirement—the enhanced population of the higher of two suitably separated quantum states—can be met by exciting spin levels in certain crystals at low temperatures, and that very low noise figures can be achieved by those solid-state masers.

8. Scattering of molecular beams

Already in 1926, before the diffraction of electron beams had been discovered, Stern suggested that molecular beams might be used to test de Broglie's surmise that a stream of particles of momentum p would behave like a wave train of wavelength $\lambda = \boldsymbol{h}/p$. Helium seemed to offer the best chances; its atoms would suffer little adsorption on the diffraction grating, and the average wavelength was conveniently large, about 10^{-8} cm. An attempt with ruled gratings was inconclusive, but clear diffraction patterns were obtained (Estermann and Stern 1930) with cleaved lithium fluoride surfaces which are natural cross-gratings, chequer boards with the large fluorine ions protruding over the small ones of lithium.

In order to verify de Broglie's equation $\lambda = \boldsymbol{h}/p$ quantitatively, Estermann *et al.* (1931) measured the diffraction angle with helium atoms of well-defined velocity, obtained by passing the beam through two rotating discs with radial slots (rather as in Fizeau's measurement of the speed of light). A sharp diffraction maximum was indeed found, but its position differed by 3 per cent from the computed one. Frisch would have been satisfied, but not Stern! He insisted on checking everything, and finally it was found that the dividing circle of the lathe had been in error: 408 slots had been cut instead of 400. The remaining discrepancy of 1 per cent was felt to lie within the limits of error.

Puzzling dips were found in the diffraction spectra, and even more spectacular ones in the specular reflection (Estermann and Stern 1930) when the crystal was turned in its own plane, something which ought not to affect the reflection at all. Frisch found some empirical rules governing the behaviour of those dips, but the reason was found by Lennard-Jones and Devonshire (1935): they showed that the intensity of the reflected or diffracted beam dropped whenever atoms could be diffracted straight into an adsorbed state. In such a state the atom can be pictured as skidding along the surface and at the same time oscillating normally to it in the field of a weak attractive force. From Frisch's results it then followed that the helium atom could exist in two quantized states in that field, corresponding to adsorption energies of 2·5 and 5·6 MEV (58 and 129 cal/mole) which would have been difficult to detect in any other way.

The scattering of molecular beams in a gas is also largely a wave phenomenon, particularly at small angles. The force between two gas molecules falls rapidly with increasing distance but never becomes zero; hence as the arrangement is made sensitive to smaller deflections, the observed mean free path would become smaller, eventually approaching zero, according to classical physics. Indeed the earliest beam experiments showed a mean free path less than half that obtained from kinetic gas theory and related mainly to big deflections. However, the indefinite decrease with decreasing scattering angle disappears when the calculations are done with quantum theory: for any force dropping faster than $1/r^2$ (the Coulomb force) the diffraction pattern has a finite maximum for very small scattering angles.

This has indeed been confirmed by molecular-beam experiments (see, e.g., Frazer and Broadway 1933).

Our present forays into outer space have revived interest in the action of atoms and molecules on solid surfaces (exchange of energy and momentum, sputtering etc). Space vehicles move at about 10^6 cm/sec, several times faster than gas molecules at practicable temperatures. Kistiakowsky and Slichter (1951) have experimented with high oven pressures and a Laval nozzle for an oven slit, of the kind that is used in gas turbines. In such a nozzle, much of the random motion of the molecules is converted into forward motion of the gas, and one gets a jet of molecules moving several times faster, but with less relative random motion, than the oven temperature would warrant. Another way of getting fast molecules is to ionize them, accelerate them through a few volts, and then neutralize them by passage through a gas with low ionizing energy.

9. Application to optical spectroscopy

Much of molecular-beam research is done in competition with optical spectroscopy, but molecular beams can also be used to aid it. The Doppler effect, one of the chief sources of line broadening, can be almost eliminated by using the light emitted at right angles to a molecular beam, excited either by resonance radiation or by bombardment with electrons; the latter transfer some momentum to the molecules they excite, but not enough to be troublesome. Even with a crudely collimated beam the random motion of the atoms at right angles to the beam can easily be reduced to one tenth the motion in the gas, corresponding to a temperature of a few degrees Kelvin. Incidentally, the recoil a sodium atom suffers on emitting its resonance line has been observed by Frisch (1933). It is also possible to get very sharp lines in absorption, by using a molecular beam as an absorber (see, e.g., Jackson and Kuhn 1935).

I hope this sketchy survey has given the reader some impression of the wide scope of molecular-beam research. It is no longer a family affair: new groups are springing up in all parts of the world. High vacuum today is a matter of course; the technique probably contains less 'know-how' than many other parts of experimental physics. With fresh minds at work, new and exciting uses for molecular beams will no doubt be found.

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The structure of the atomic nucleus

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Summary

The properties of the atomic nucleus are summarized and shown to be consistent with a description in which the nucleus is regarded as an assembly of elementary particles (neutrons and protons) bound together by means of nuclear forces. Because of the formidable difficulty of giving a full theoretical treatment of this many-body problem, nuclear models are introduced which can account in a semi-quantitative fashion for many of the nuclear properties. Finally, a discussion is given of the way in which these models are related to one another and to a ' true ' description of the nucleus.

1. Introduction

It is now nearly fifty years since Rutherford firmly established from the way in which charged particles are scattered by matter that an atom consists of a massive central nucleus surrounded by a cloud of electrons, the total electric charge of the electrons being equal and opposite to that of the nucleus so that the atom as a whole is electrically neutral. He further concluded that whereas the electron cloud has a spatial extension of the order 10^{-8} cm, the nucleus is a very small structure confined to a region of dimensions a few times 10^{-13} cm. Thus in most physical and chemical processes the nucleus can be regarded as a point charge; only the electrons take part in the interactions.

Since Rutherford's work a great deal of effort has been expended in investigating the nature and structure of the atomic nucleus. In the space of an article of this size, however, it is quite impossible to give any account of the ingenious and painstaking experiments that have been performed. Suffice it to say that the apparatus has varied from the crudest 'string and sealing-wax' arrangements to the gigantic high-energy machines such as the bevatron and cosmotron. Some experiments have been extremely inaccurate, others of the utmost accuracy, but all have contributed to a now well-established body of knowledge about the nucleus.

2. Properties of the nucleus

2.1. *Mass*

Nuclear masses are roughly integer multiples of the mass of the proton (the hydrogen nucleus) the integer being denoted by A and referred to as the mass number. Nuclei with values of A from 1 up to 255 have been observed although a great many of them do not occur naturally.

2.2. *Electric charge*

The charge of a nucleus is always an integer multiple (Z) of the fundamental unit $e = 4{\cdot}805 \times 10^{-10}$ e.s.u. and is written Ze where Z (the atomic number) is also equal to the number of electrons in the surrounding electronic cortège. Now it is frequently found that several nuclei having different values of A will have the same Z. The corresponding atoms have essentially the same chemical properties since these are determined by the electronic structure which will, of course, be the same in each case. Such nuclei are called *isotopes* and may have very different nuclear properties.

2.3. *Size and shape*

Most nuclei are nearly spherical in shape with a radius R given approximately by the formula

$$R = r_0 A^{1/3}$$

with $r_0 \approx 1{\cdot}2 \times 10^{-13}$ cm. The density distribution $\rho(r)$ is not constant however, and varies in the radial direction in the manner shown in fig. 1. This means that there is no sharply defined surface to the nucleus and that the radius R must be defined in some average sense. A suitable definition is that R is the root mean square radius, given mathematically by

$$R^2 = \left[\int_0^\infty \rho(r) r^4\, dr\right] \Big/ \left[\int_0^\infty \rho(r) r^2\, dr\right].$$

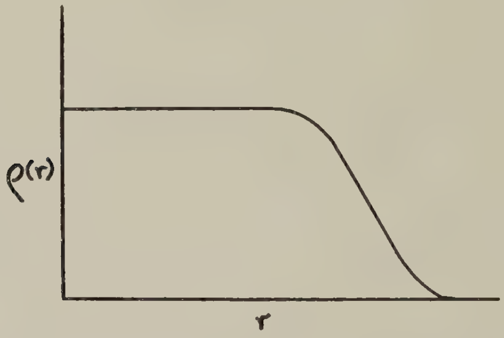

Fig. 1. The density distribution $\rho(r)$ of a typical nucleus plotted as a function of r (arbitrary units).

2.4. *Spin*

In classical mechanics the idea of a spinning body is perfectly familiar and there is no restriction on the angular momentum (or spin) that it may possess. However, when we are dealing with a structure as small as the nucleus, then the angular momentum will also be very small and quantum mechanical effects become important. Quantum mechanics restricts the angular momentum of a system to be an integer or half-integer multiple of a natural unit of angular momentum $h/2\pi$ (often denoted by $\hbar$), where h is Planck's quantum constant having the numerical value $h = 6{\cdot}624 \times 10^{-27}$ erg-sec.

Experimentally it is found that a great many nuclei have an intrinsic spin $I\hbar$ with I, the quantum number, taking all integer and half-integer values up to 9/2. It is also found that the value of I is intimately related to the value of Z and $A-Z$ for the nucleus under consideration according to the following table:

Z	$A-Z$	I
Even	Even	0
Odd	Odd	Integer
Even	Odd	Half-integer
Odd	Even	Half-integer

Thus, when A is even the spin is zero or integer and when A is odd the spin is half integer. We shall shortly see that these relations are one of the deciding features in distinguishing between different possible theories of the nucleus.

2.5. *Magnetic moment*

Since the nucleus is charged it is to be expected that if it is also spinning then, because a circulating charge is an electric current, it should have a magnetic moment. This is indeed found to be the case; all nuclei with I greater than zero have a magnetic moment whose value is measured in nuclear magnetons μ_0 where $\mu_0 = e\hbar/2Mc$ (M = proton mass, c = velocity of light).

2.6. *Electric quadrupole moment*

The quadrupole moment of a distribution of electric charge is a measure of the extent to which the charge distribution deviates from spherical symmetry. If a nucleus is ellipsoidal in shape, for instance, then the quadrupole moment Q is given by the expression

$$Q = \tfrac{4}{5} Z R^2 (\Delta R/R)$$

where R is the average nuclear radius and ΔR is the difference between the major and minor axes of the ellipsoid. Q can be both positive or negative corresponding to the ellipsoid being prolate or oblate (i.e. like a rugby football or a flying saucer) respectively. Quadrupole moments are found to vary considerably in size from almost zero to values corresponding to $\Delta R/R \approx 0{\cdot}4$. This is strikingly shown in fig. 2 where observed values of Q/R^2 for odd nuclei are plotted against Z or $A-Z$ whichever happens to be odd. The important fact is brought out by this diagram that Q changes sign for certain values of Z or $A-Z$, namely, 2, 8, 20, 28, 50, 82, 126. This fact is the first of many examples of the observation that nuclei for which Z or $A-Z$ are equal to one or other of these numbers have distinctive properties associated with them.

2.7. *Radioactive decay*

The fact that many nuclei are radioactive, that is, change from one nucleus to another or from one state to another, and at the same time emit radiation of some kind, is now commonplace knowledge. Three types of radiation are emitted, known as α-rays, β-rays and γ-rays. We shall consider first α- and β-rays which are both particle emissions, the former being helium nuclei ($Z=2$, $A=4$) and the latter electrons or positrons (a positron is a particle having the same mass as an electron but positively charged).

In α-decay it is always observed that the α-particles are emitted either with one definite energy or sometimes in several groups, each group having a well-defined energy. This behaviour is very significant and can easily be understood in terms of quantum mechanics. We have already mentioned that quantum mechanics limits the possible values of the angular momentum of a system to be integer to half-integer multiples of $\hbar$; similarly it restricts the possible values that the energy of a system can take to discrete values. Normally a nucleus is in its state of lowest energy; however, just as a liquid can be given energy by heating it, so a nucleus can be given energy by, for example, bombarding it with other nuclear particles. The nucleus is then raised into an *excited state* and it is the energy of these states that quantum mechanics restricts to discrete values.

Such states are conventionally indicated by an energy level diagram of the type shown in fig. 3 (*a*). Horizontal lines represent possible states and the distances between them indicate the energy separations between the states. A, the state of lowest energy, is referred to as the ground state and the remainder as excited states.

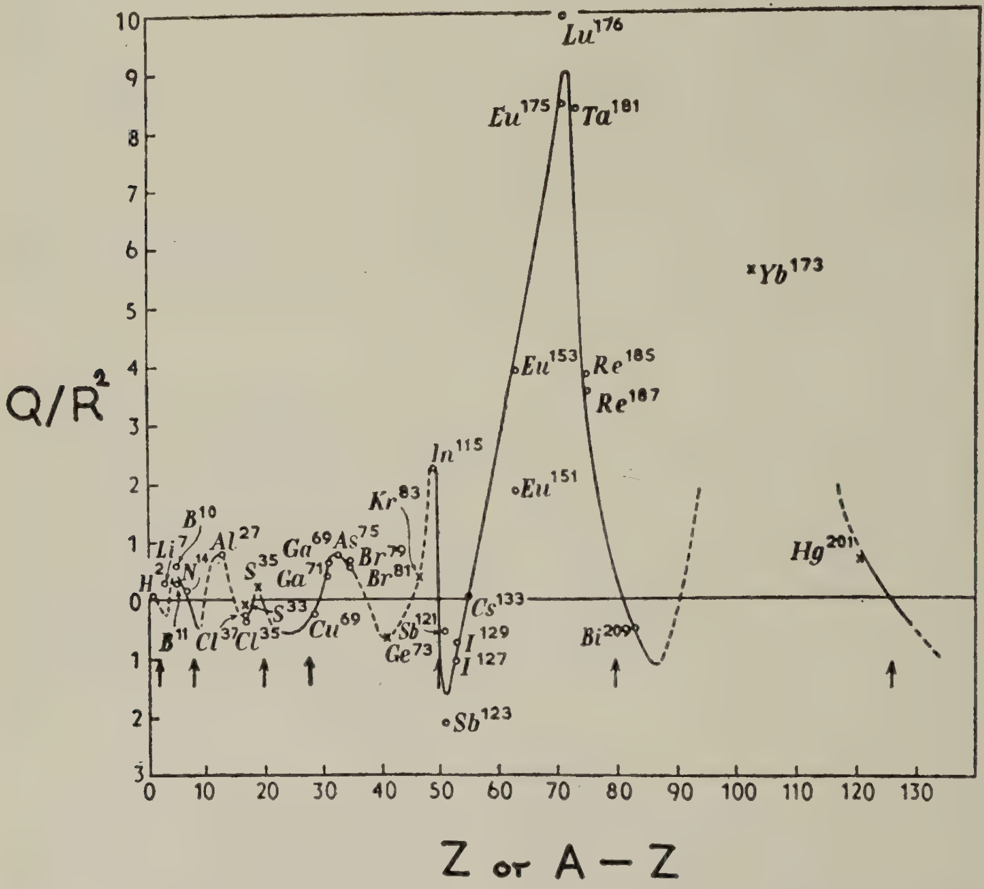

Fig. 2. Experimental values of Q/R^2 for odd A nuclei plotted as a function of Z or $A-Z$ whichever is odd (from Townes, Foley and Low, *Phys. Rev.*, 1949, 76, 1415).

Now consider the α-decay of nucleus X to nucleus Y shown in fig. 3 (*b*). Since the decay takes place at all it implies that X has more energy than Y, hence it is drawn higher in the diagram. If X was in its ground state then two possible transitions could be made; these are indicated by the two arrows. Since the transitions are between states of definite energy it is clear that since energy is conserved the α-particles must be emitted with two definite energies and, in observing a large number of decays of the X nuclei, the particles will be divided into two energetically different groups, the intensities of each group being proportional to the probabilities of the two transitions.

In the light of this argument it is then of considerable significance that in β-decay, the electrons (or positrons) are emitted with a spread of energies up to a certain maximum. How can this behaviour be reconciled with the foregoing discussion? The answer is that in β-decay *two* particles are in fact emitted, the electron (say) and a neutrino. The latter is a curious particle which has zero electric charge and is believed to have zero mass. (In spite of this, relativity theory still allows it to take away energy and momentum.) Because of its properties it is extremely difficult to detect and it is only within the last year or so that its presence has been directly confirmed by experiment; previously its existence was only inferred. Given such a particle, then the electron and neutrino will share the 'definite' energy released in a transition between two nuclear states and electrons will be observed with all energies up to the maximum possible in the particular transition considered.

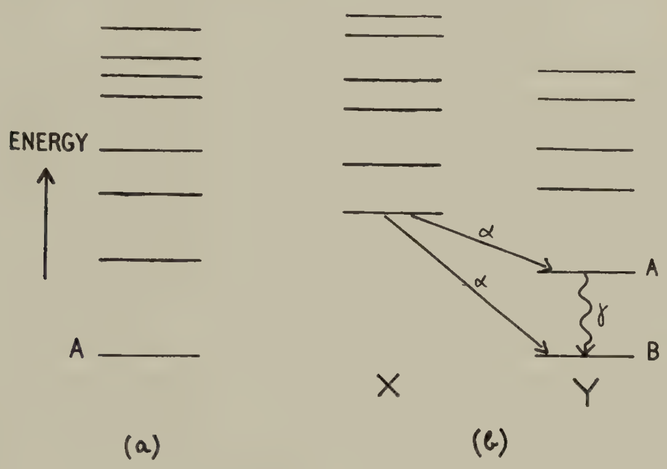

Fig. 3. (*a*) Nuclear energy levels. A is the ground state. (*b*) α decay from nucleus X to nucleus Y and γ-transition between states A and B.

In α- and β-emission, since electric charge is carried away from the nucleus, the charge Z of the nucleus must also change. This means that the final nucleus is completely different from the initial one. On the other hand, γ-rays are very short wavelength electromagnetic waves and although they carry away energy, they do not carry away any charge. In this case, then, there is a change in the state of the nucleus, but that is all. In fig. 3 (*b*) a γ-transition from state A to B in nucleus Y is indicated by the wiggly line between A and B. Here again we expect the γ-ray to have a definite energy equal to the difference between that of states A and B; this is in agreement with experiment. The situation is completely analogous to that obtaining in the case of emission of light by atoms, where again there is quantization of energy levels and only certain wavelengths (or energies) are seen.

2.8. *Regularities*

It was pointed out earlier that electric quadrupole moments change sign at nuclei for which Z or $A-Z$ equals 2, 8, 20, 58, 50, 82, 126. There are many other indications that nuclei associated with these so-called 'magic numbers' have distinctive properties. Among them may be mentioned:

(i) Such nuclei have much higher cosmic abundances.
(ii) Nuclear binding energies are greatest at magic numbers.
(iii) The excitation energies of first excited states are greatest at magic numbers.

Perhaps the most striking 'magic' nucleus is ^{208}Pb which terminates three of the well-known radioactive series and which has $Z=82$, $A-Z=126$.

3. COMPONENTS OF THE NUCLEUS

In the same way that an atom is described in terms of a nucleus surrounded by a certain number of electrons, so we should like to describe the nucleus as a composite structure formed from a number of fundamental particles. In the early days, the most well-known particles were the proton (i.e. the hydrogen nucleus) and the electron. An obvious suggestion was then that a nucleus with atomic number Z and mass number A had as its constituents, A protons and $A-Z$ electrons. This ensures that the charge is Ze and that the mass is approximately correct (remember that the mass of an electron is nearly 2000 times smaller than that of a proton, so that the nuclear mass would be roughly that of A protons). Further, the emission of electrons in β-decay seems to support this hypothesis. However, there are two strong arguments against it.

Firstly, in quantum mechanics there is a very famous result known as Heisenberg's uncertainty relation. This relation tells us that if we confine a particle to a small region of space, then there is uncertainty as to its momentum; the particle acquires a 'zero-point' energy, a sort of violent wobbling whose magnitude depends on Planck's quantum constant h and is inversely proportional to the size of the region to which the particle is confined. A simple calculation shows that an electron confined to a volume of nuclear size would acquire a zero-point energy of the order 20 MeV† so that the Coulomb force between the protons and electrons, and this is the only force acting between them, would be quite unable to keep the electrons in the nucleus.

Secondly, both the proton and electron have spin $\frac{1}{2}\hbar$ and the rule for addition of angular momentum in quantum mechanics is that an odd number of half-integer spins gives a resultant half-integer spin, whereas an even number gives an integer spin. Now on the above picture for a nucleus (A, Z) the total number of particles (protons and electrons) is $A+(A-Z)=2A-Z$. Thus the nucleus should have half-integer or integer spin according as $2A-Z$ is odd or even. This is manifestly in disagreement with observation which is that the spin is determined by whether A is odd or even (see § 2).

Thus on the above two counts the proton–electron picture of the nucleus is untenable. However, in 1932 Chadwick discovered the neutron. This is an electrically neutral spin $\frac{1}{2}\hbar$ particle having approximately the same mass as the proton and is clearly an excellent candidate as a nuclear building block. The nucleus can now be regarded as made up of Z protons and $N=A-Z$ neutrons. In this case the uncertainty relation does not give unreasonable results for the zero-point energy of the component particles since they are so much heavier than an electron. Further, the total number of particles in the nucleus is A so that the correct relationship between A and nuclear spin is obtained.

Of course, β-decay is now more difficult to understand since there are no longer any electrons in the nucleus. The current explanation of this is common to many similar processes involving elementary particles, namely that a neutron, for example, can *spontaneously* change into a proton together with an electron and a neutrino (symbolically $n \to p + e + \nu$). It is to be noticed here that apart from needing a neutrino to account for the energy spread of electrons in β-decay we also need it in the process to conserve angular momentum. For, if the neutron decay process had been $n \to p + e$ (without a neutrino) then the even number of particles $(p + e)$ on the right-hand side, having integer spin, would not match the spin $\frac{1}{2}\hbar$ of the neutron. On the other hand, if the neutrino has spin $\frac{1}{2}\hbar$ (for which there is now considerable evidence) then angular momentum can be conserved.

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† 1 MeV (million electron volts) is the energy necessary to accelerate an electron through 1 million volts. It is a convenient unit for the measurement of nuclear energies and is equal to $1{\cdot}6\times10^{-6}$ ergs.

4. Nuclear binding and nuclear forces

Given that the particles from which a nucleus is constructed are neutrons and protons, we have next to consider the way in which they are bound together. The magnitude of this binding can be estimated very easily for any given nucleus (A, Z) by comparing the actual mass of the nucleus $M(A, Z)$ with the sum of the masses of its constituent particles. That is, we compare $M(A, Z)$ with $ZM_p + NM_n$ where M_p and M_n are the masses of the proton and neutron respectively. In the case of a stable nucleus $M(A, Z)$ is always less than the sum of the masses of its constituents. Bearing in mind Einstein's relation between mass and energy $E = Mc^2$, it follows that energy has to be provided in some form in order to break up the nucleus and separate its components. Conversely, if we tried to build the nucleus up from neutrons and protons, then energy would be released in the process. The quantity $B = \{ZM_p + NM_n - M(A, Z)\}c^2$ is known as the binding energy of the nucleus. A more significant quantity to discuss, however, is the

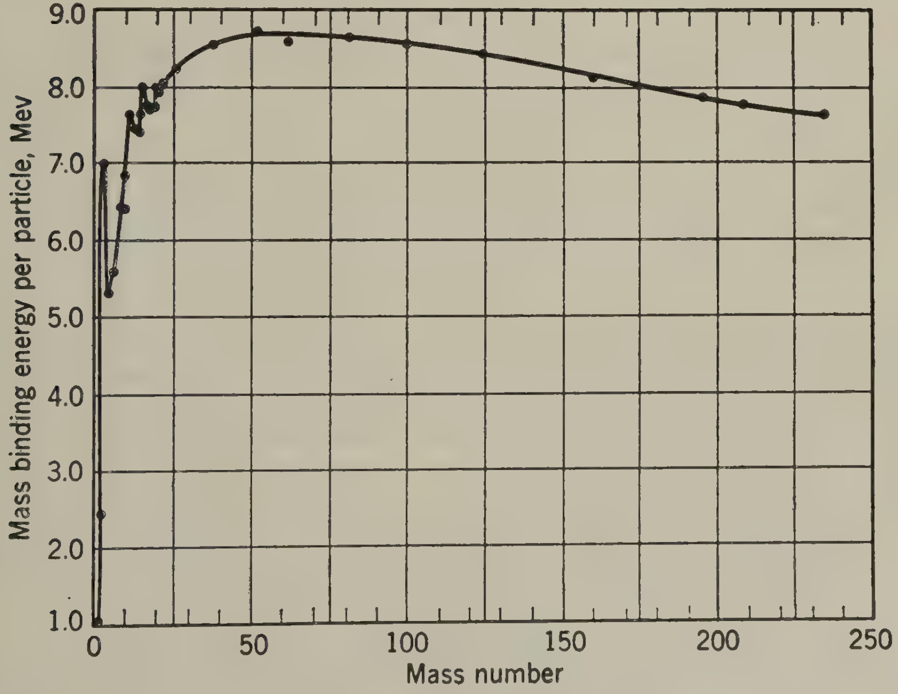

Fig. 4. Binding energy per particle (in MEV) plotted against A.

binding energy per particle which is given by B/A. Its variation with A is shown in fig. 4 and a most important fact is demonstrated in this diagram, namely, that the binding energy per particle is approximately constant apart from a few very light nuclei. Its average value is about 8 MeV.

The obvious interpretation is to describe the binding of nuclei in terms of forces between nucleons (this is a collective name used to describe both neutrons and protons). These forces must be predominantly attractive in order to hold the nucleons together and furthermore must have a very short range of the order 10^{-13} cm so that nuclei have their observed sizes. Of course, the easiest way to determine the nature of these forces is to consider the interaction between only two nucleons. This can be effected by investigating the way in which neutrons and protons are scattered by each other and also the properties of the deuteron, the latter being the simplest possible nucleus consisting of a neutron and a proton only. Such investigations have been carried out and indicate quite definitely that nuclear forces are indeed of the required nature and that, in addition, they are charge independent. That is to say, they are essentially independent of whether the interacting particles are neutrons or protons. In addition, the experiments can be refined so that measurements are made of the way the forces behave when the spins of the nucleons are oriented in different directions relative both to each other and to their relative orbital angular momentum. There is clear evidence that the strength of the force depends on whether the spins of the two nucleons are parallel or anti-parallel to each other and it also seems highly probable that the force depends on how the spins are oriented with respect to the orbital angular momentum. The latter behaviour is referred to as spin–orbit coupling. One final property of the inter-nucleon interaction also emerges, namely that when two nucleons approach very closely to each other ($r < 0{\cdot}4 \times 10^{-13}$ cm) then the force between them becomes highly repulsive.

This repulsive behaviour provides an explanation of two facts which have been presented about the nucleus. Firstly, the repulsion prevents all the nucleons in the nucleus from collapsing inwards to form a structure whose size would be essentially independent of the number of nucleons. Rather, we now have the situation that the nuclear volume should be proportional to the number A, of nucleons and this requires that the nuclear radius should be proportional to $A^{1/3}$ in accord with observation. Further, detailed calculations of this effect show that the average separation of two nucleons in the nucleus is of the order $1{\cdot}5$–$2{\cdot}0 \times 10^{-13}$ cm, corresponding to nuclei radii of the observed order of magnitude. Because of the magnitude of this average separation between nucleons it follows that in a large nucleus any one nucleon interacts with a comparatively small number of its neighbours since the nuclear force only extends over a distance of the same order of magnitude as the separation. Thus the binding energy of the nucleus, which reflects the total attractive potential energy between nucleons, is proportional to the total number of particles; that is, B/A is approximately constant as observed. If, on the other hand, the nucleons all collapsed inwards then each nucleon would interact with all the others so that the binding energy would be proportional to the total number of *pairs* of nucleons, that is, to $A(A-1) \approx A^2$ in complete disagreement with experiment.

Summarizing then, the nucleus is a many-body system consisting of Z protons and $N = A - Z$ neutrons interacting with each other through a force having a range of the order 10^{-13} cm which is predominantly attractive at large distances and repulsive at short distances. Furthermore, the force depends not only on the distance between two nucleons but also on the orientation of their spins both to each other and to their relative orbital angular momentum. The nucleus is indeed a highly complex structure.

5. Nuclear models

To obtain an exact theory of such a complicated structure is not possible —at least with our present mathematical techniques. Therefore, as with many other insoluble problems in physics, the approach adopted in order to give some theoretical account of the more detailed nuclear properties has been to devise models of the nucleus which may be regarded as crude approximations to the true state of affairs. Such models are suggested by known properties of the nucleus (e.g. the existence of magic numbers) and by other semi-classical considerations. Furthermore, a model must be amenable to theoretical treatment so that results can be calculated with it and compared with experiment. The success of a model is then judged by the extent to which theory and experiment are in agreement. In the following the two most successful nuclear models will be described, the Shell Model and the Collective Model. Both have had considerable success in accounting for various nuclear properties and have thrown a great deal of light on the real state of the nucleus.

5.1. *The nuclear shell model*

The idea of a shell model for the nucleus is at once suggested by the existence of magic numbers, that is, by the existence of certain nuclei which are particularly stable and might even be approximately described as *inert*. In atomic theory inert elements (the noble gases) are well known as structures which are very stable and whose stability lies in the fact that certain shells of electrons in the atom have been filled. This 'filling' arises because electrons have spin $\frac{1}{2}\hbar$ and therefore obey the Pauli exclusion principle, a principle which requires that in any quantum mechanical system no more than one electron can exist in any one state of motion (or quantum state). Now in an atom the various states arise because the electrons are moving in the central Coulomb field of the nucleus and they are labelled by quantum numbers, conventionally denoted by n, l, m_l and m_s. The relationship of these quantum numbers to physical concepts is roughly as follows.

In quantum mechanics, a material particle cannot in general be completely localized; the uncertainty relation referred to in § 3 is a statement of this fact. Thus, in an atom, an electron may be regarded as smeared out into a charge cloud around the nucleus. The way in which the density of the cloud varies with radial distance from the nucleus is then determined by the quantum number n, whilst its angular variation is dependent on l and m_l. Furthermore, in a given quantum state, the energy of the electron depends on n and l, whilst the magnitude of its orbital angular momentum about the nucleus is given approximately by $l\hbar$ and its direction is determined by m_l. The three quantum numbers n, l and m_l are restricted to integer values and for a state with energy $E(n, l)$, m_l can only take the $(2l+1)$ values $l, l-1, \ldots, -l$. Finally, m_s can take the values $\pm\frac{1}{2}$ corresponding to the two quantum mechanically allowed spin orientations for an electron.

Electrons are said to be in the same shell if they have the same values of n and l, that is, the same energy in the Coulomb field of the nucleus. Clearly, since for given n and l, m_l can take $2l+1$ values corresponding to $2l+1$ possible orientations of the electron orbit and two electrons ($m_s = \pm\frac{1}{2}$) can go into each of these states, then a filled shell contains $2(2l+1)$ electrons. Now the ground state of an atom is its state of lowest energy and is obtained in terms of the foregoing description by filling up each level $E(n,l)$ in turn, starting from the lowest, until all Z electrons are accommodated. If as a result of this process the addition of the last electron just fills a shell, then the atom can be shown to be very stable and it is in this way the noble gases are accounted for.

One might hope for a similar explanation of the magic numbers which occur in the case of nuclei. However, whereas in the atom the different energy levels arise very simply, since the electrons are orbiting in the central Coulomb field of the nucleus, within the nucleus the situation is at first sight very different. For here it is not at all obvious how such a central field could arise, since there is no central massive particle to provide it. Nevertheless, a plausible explanation can be given.

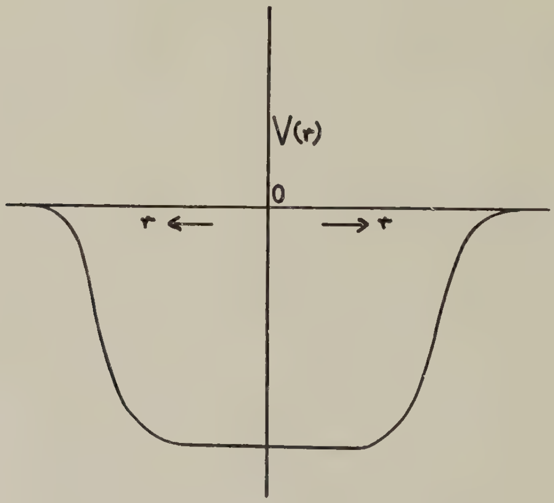

Fig. 5. Nucleon potential energy $V(r)$.

Suppose we consider the potential energy $V(r)$ experienced by a nucleon as it moves from one side of a nucleus to the other. Away from the nucleus the potential is zero if we forget about Coulomb forces; as it approaches near to the nuclear surface it then experiences the attractive part of the potential due to the nearest nucleons so that the potential goes negative (fig. 5). Then well inside the nucleus the potential remains roughly constant on average at a certain negative value, since a given nucleon is only interacting with a few nucleons at a time and the situation is approximately independent of its position. Finally, emerging from the other side, the potential increases to zero again. Thus, as a first approximation we might regard the nucleons as moving independently of one another within a potential 'well' of the type shown in fig. 5, this well being the average field experienced by any one nucleon in the presence of all the others. In this well there will be a series of energy levels $E(n,l)$ whose relative spacings depend on the depth, size and shape of the well. The arrangement of these levels for such a well with a depth of about 40 MEV and radius 8×10^{-13} cm is shown on the left of fig. 6†. In using the exclusion principle to control the filling of these levels it must be remembered that the principle only applies to *identical* particles so that a neutron in a level does not exclude a proton from occupying the level or vice versa. It can be seen that certain large gaps occur in the energy level scheme and that the total number of neutrons or protons that can be included below the lower gaps (remember $2(2l+1)$ particles can go into each level) correspond exactly with the magic numbers 2, 8, 20. However, the higher numbers do not appear.

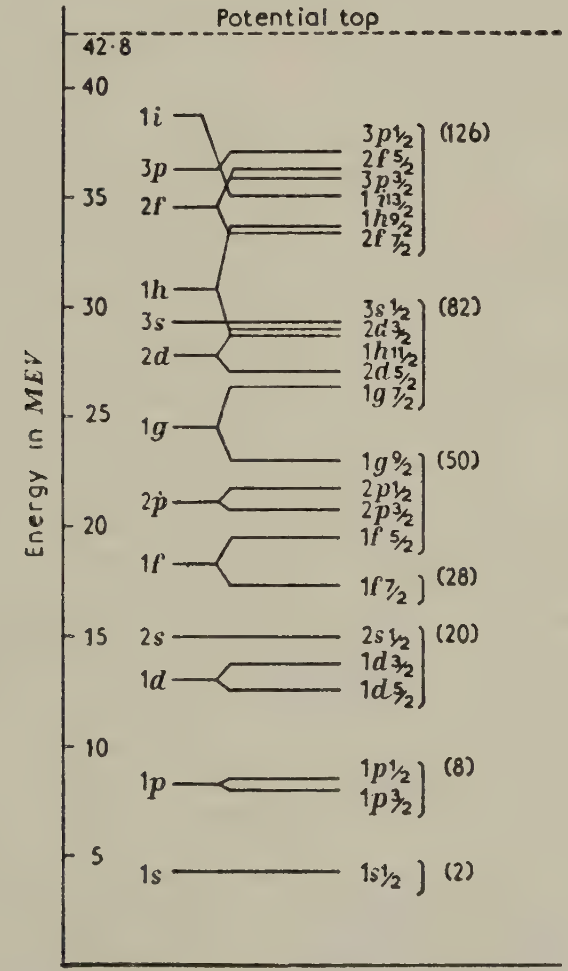

Fig. 6. Energy levels in a potential well of the type shown in fig. 5. The notation used is as follows: States with $l \pm 0$, 1, 2, 3, . . . , etc. are denoted by the letters *s*, *p*, *d*, *f*, *g*, . . . , etc. The number to the left of each letter is the value of *n* and to the right, the value of *j*. On the extreme right of the diagram are the magic numbers representing the number of nucleons that completely fill all the preceding levels.

† The notation used in the diagram is described in the figure caption.

It was not until 1948 that Mayer and Jensen showed how the higher numbers could be produced in a natural way. They pointed out that because of the spin dependence of nuclear forces, it is to be expected that a nucleon in the nucleus experiences a strong spin–orbit force such that its energy depends on whether its spin is parallel or anti-parallel to its orbital angular momentum $l\hbar$. That is, its energy is different according as its total angular momentum $(j\hbar)$, which is the vector spin of its sum and orbital angular momentum, is $(l+\frac{1}{2})\hbar$ or $(l-\frac{1}{2})\hbar$. This leads to a ' splitting ' of the energy levels, the energy of any level now depending on n, l and j and to agree with experiment the sign of the spin–orbit coupling is such that the state of highest j lies lowest. Simple arguments can be used to show that this splitting should be approximately proportional to $(2l+1)$ leading to very large splittings for states of high l. The level scheme obtained because of this effect is shown on the right-hand side of fig. 6. Bearing in mind that because of the exclusion principle a level j can be occupied by a maximum of $(2j+1)$ particles it can be seen that all the magic numbers now occur in a natural way immediately below relatively large gaps. Of course, the larger a gap, the more stable is the immediately preceding nucleus since it requires more energy to excite it. What has happened as a result of introducing the spin–orbit coupling is that for the higher groups of levels the splitting of the state with the highest orbital angular momentum is so great that the lower component of the resulting doublet joins the next lowest group of levels.

We now have a basic model and next have to consider to what extent it can account for other nuclear properties. Its success, in fact, is phenomenal. Consider first of all the question of nuclear spins. Here we have to take account of the fact that the nucleons do not move completely independently of one another. Although the forces between the nucleons are partially accounted for by the average potential well that has been introduced, there are still residual nuclear interactions representing the difference between the well potential energy and that actually experienced by a nucleon. From the known nature of nuclear forces it is to be expected that these interactions are such that like nucleons in the different levels tend to pair off with one another so that their total angular momentum is zero. Under these circumstances there is no contribution to the angular momentum of a nucleus having an even number of neutrons and an even number of protons since their orbits and spins are all paired off. This is in agreement with the observation that the so-called even–even nuclei have spin zero (see § 2). However, for an odd A nucleus in the last neutron or proton energy level to be filled there is either an odd neutron or an odd proton which is unpaired. This is the only particle contributing to the nuclear spin, which should therefore be equal to the angular momentum $j\hbar$ of this nucleon. Thus O^{17} has 8 protons and 9 neutrons; the last odd neutron has $j=5/2$ and this is the observed spin of O^{17}. Similarly Co^{59} has 27 protons and 32 neutrons; the last odd proton has $j=7/2$ which is again in agreement with the experimental spin value. It is, however, significant that in the cases where high spins are to be expected (e.g. when the $j=11/2$ state is being filled) they are not observed. The reason for this is that the residual pairing forces are much more effective in states of high angular momentum so that particles like to pair off in such states, even at the expense (energetically) of lifting a nucleon out of a lower angular momentum state in order to do this. In this case the nucleus has its spin equal to the lower angular momentum. Taking this into account, there are only very few odd A nuclei whose spins are in disagreement with this simple shell model prediction, and in all such cases there are extenuating circumstances. As far as odd–odd nuclei are concerned (i.e. those with odd Z and odd N) the shell model in the form so far described makes no prediction other than that the spin should be integer.

The shell model also predicts the parities of nuclei. Parity has been a much discussed concept recently; roughly speaking it is a description of the way the nuclear properties change when investigated in a mirror world, that is, in a world in which the three coordinate axes have been reflected through the origin. It is sufficient for our purposes to note that it is said to be either 'even' or 'odd' according as the nuclear wave function remains the same or changes sign under reflection of axes. On the shell model the parity of an odd A nucleus is dependent

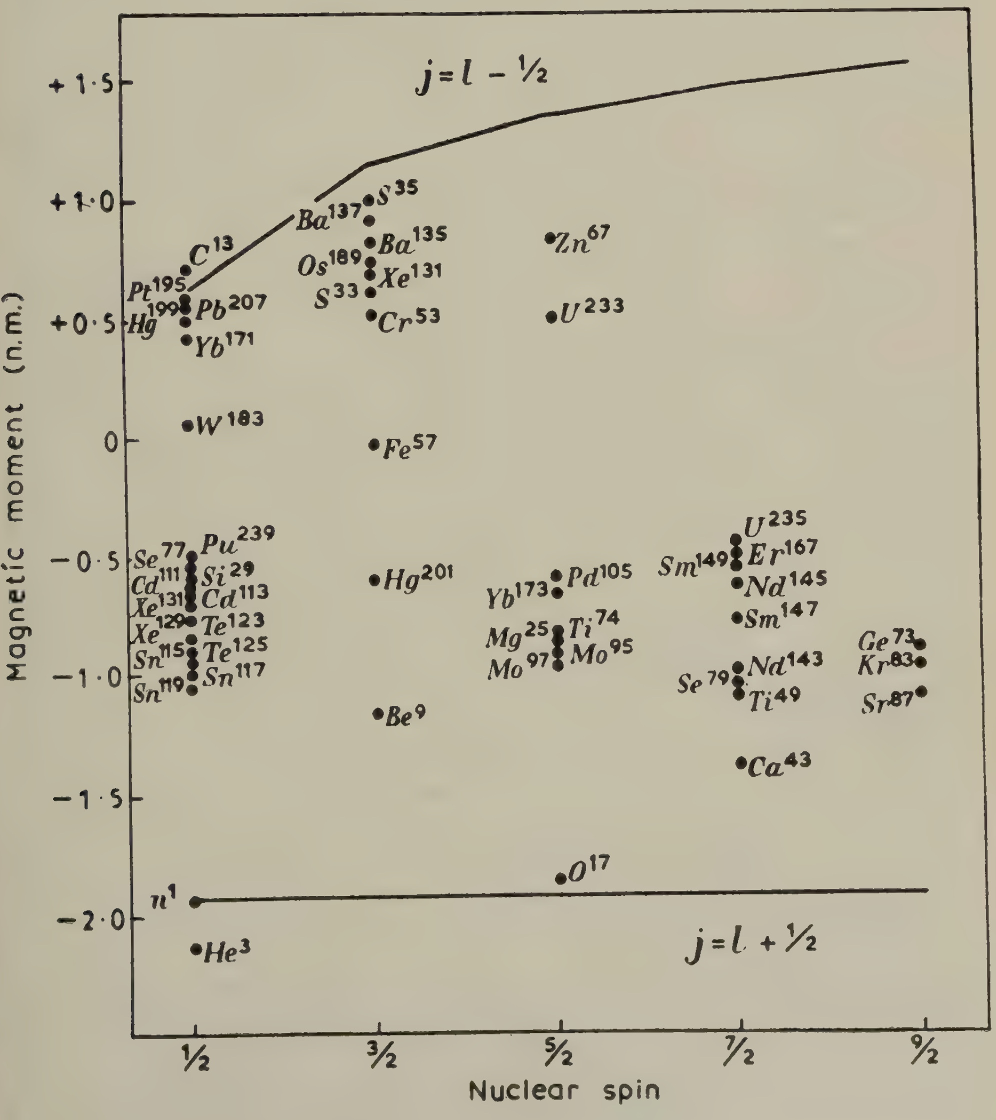

Fig. 7. Schmidt diagram for odd neutron nuclei.

on the l value for the last odd nucleon, being odd if l is odd and even if l is even. Again theory and experiment agree rather well.

Perhaps one of the most striking successes of the shell model is its ability to describe the general behaviour of the nuclear magnetic moments of odd A nuclei. For these nuclei the magnetic moment is predicted to be that of the last odd neutron or proton as the case may be. This is because the contributions to the magnetic moment from the other nucleons exactly cancel due to the pairing effect. The theoretical and experimental results are presented in the so-called Schmidt diagrams (figs. 7 and 8) for odd neutron and odd proton nuclei. On the horizontal axis is plotted the nuclear spin and on the vertical axis the magnetic moment. The full lines correspond to the theoretical predictions for the two possibilities $j = l \pm \frac{1}{2}$ and of course only have any meaning at the allowed half-integer spin values at which are plotted the experimental points. The qualitative agreement is surprisingly good and can be considerably improved by refining the shell model in various ways.

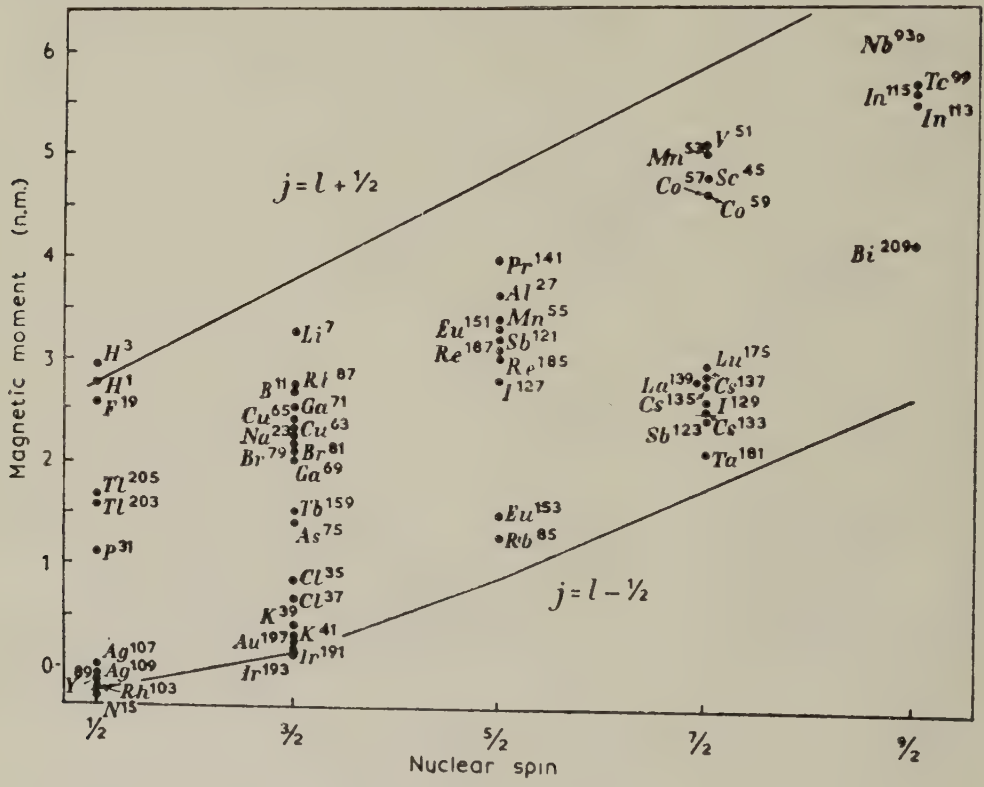

Fig. 8. Schmidt diagram for odd proton nuclei.

When we come to consider nuclear electric quadrupole moments, Q, however, there is violent disagreement. This arises mainly in the approximate regions $150 < A < 185$, $225 < A$ where the experimentally observed values of Q are sometimes as much as 30 times larger than the shell model predictions. These predictions are calculated in the same way as the magnetic moments by attributing Q to the last odd particle. In particular, since a neutron has zero charge, an odd neutron nucleus is expected to have $Q=0$; on the contrary this is never observed. The fact that quadrupole moments having many times the single particle values occur is significant since it indicates that many particles are contributing and that we have here a collective effect of some kind. This will be discussed in detail a little later.

Of the other nuclear properties, the nature of excited states must be considered. In a nucleus such as O^{17} for example there are 8 protons and 8 neutrons each in a 'magic' and therefore very stable closed shell together with an odd neutron in a state with $j=5/2$. It is then to be expected that excited states could most simply arise by leaving the closed shells undisturbed and by exciting the odd neutron into one of the higher states; thus among the low excited states we should expect one with $j=3/2$ and one with $j=\frac{1}{2}$. This is in agreement with experiment and there are many cases such as this where the low-lying excited states of nuclei can be interpreted in a very simple fashion. However, the case quoted was ideal and unambiguous. In more complicated nuclei away from 'magic' closed shells the energy level scheme cannot be deduced so simply and recourse must be made to detailed calculations taking into account the residual interactions between nucleons, the energy levels then being interpreted as re-arrangements of the nucleons within incomplete shells. Where it has been possible to carry out such calculations the theoretical results have been found to be in good agreement with experiment. Right away from closed shells, however, the necessary calculations are unfeasible and it is just in these regions that the collective model suggested by Aage Bohr has had some of its successes.

Before going on to describe the collective model, one final point has to be discussed, namely, why the shell model works so well. The basis of the shell model is the assumption that the nucleons move approximately independently of one another in a smooth potential well. Plausibility arguments were given for this assumption; but these arguments completely ignored the fact that since nuclear forces are so strong and have such a short range, a nucleon moving through the nucleus would in fact experience violent fluctuations in potential energy as it passed near other nucleons in complete contradiction with our assumption of roughly independent particle motion. The presence of such effects is confirmed in a great many nuclear reactions in which a neutron, for example, strikes a nucleus and is immediately captured to form a highly excited state of the nucleus. Such capture would not take place if the neutron moved through the nucleus in the way suggested by the shell model since motion independent of the other particles implies that the nucleus is approximately 'transparent'.

The reason why, in spite of this apparently contrary evidence, the shell model might still be expected to work, was first given by Weisskopf. He pointed out that for nucleons deep down in the potential well the violent fluctuations are quite ineffective. This is because the only effect such fluctuations can have is to 'knock' a nucleon from one energy state to another nearby. However, for most particles all the nearby states are already occupied so that Pauli exclusion principle forbids such changes of state. Thus for the bulk of the nucleons the violent changes in potential have little effect. On the other hand, in the case of a nuclear reaction, the incoming particle has an energy at least 8 MeV greater than that of any nucleon in the nucleus since nucleons only fill the potential well to within 8 MeV of the top. This means that it has sufficient energy to knock particles, even deep down in the potential well, up into unoccupied states. The incoming particle therefore interacts strongly with the nucleus although the bulk of the constituent nucleons of the nucleus move approximately independent of one another.

This explanation is, of course, a considerable simplification but it is essentially correct and is confirmed by the very detailed calculations that have been performed on this problem during the past few years.

5.2. *The collective model*

It was pointed out in the previous section that the quadrupole moments of a number of nuclei are much larger than the single particle values predicted by the shell model. These nuclei are all those which are well away from closed shells and in which there is a large number of 'loose' particles, some of which have high angular momenta. Such particles will exert a high centrifugal force on the surface of the nucleus tending to distort its shape from approximately spherical to much more ellipsoidal, thus giving it a large quadrupole moment. This effect is collective in nature in that a great many nucleons participate in the overall nuclear distortion.

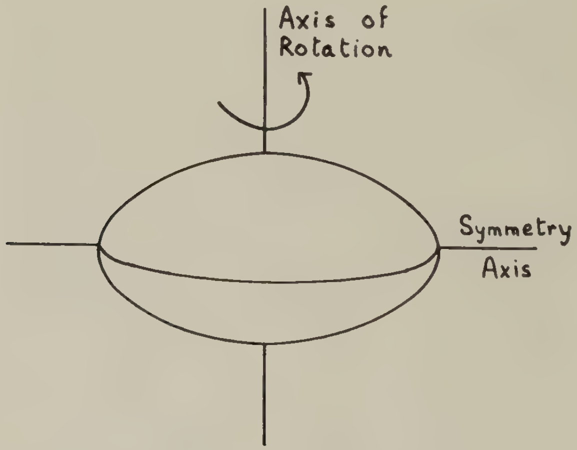

Fig. 9. Rotation of an ellipsoidal nucleus.

In the case of nuclei with such large distortions it might be expected that excited states of a rotational character should be observed. These states would be similar to those which arise in the case of diatomic molecules and are states of motion in which the nucleus rotates as a whole about an axis perpendicular to its axis of symmetry (fig. 9). Of course we must remember the limitations imposed by quantum mechanics on such rotations. Classically, the energy is given by $P^2/2\mathfrak{J}$ where $\mathfrak{J}$ is the moment of inertia of the nucleus about the axis of rotation and P the angular momentum. Quantum mechanics however restricts P^2 to taking the values $P^2 = J(J+1)\hbar^2$ where, for an even–even nucleus, J takes even integer values only. For odd A nuclei a similar but more complicated description can be given but we shall not consider these nuclei here. Thus, for even–even nuclei the allowed rotational energy levels are given by

$$E_J = \frac{J(J+1)\hbar^2}{2\mathfrak{J}}$$

with $J = 0, 2, 4, \ldots,$ etc.

It is a remarkable success of such a simple theory that energy levels agreeing very well with this formula are observed just where they are to be expected (the regions $150 < A < 185$, $A > 225$). A typical example is shown in fig. 10. An interesting feature of rotational energy levels of this kind is that the moment of inertia $\mathfrak{I}$ deduced from the level spacing is roughly equal to one-half the moment of inertia that would be obtained if the nucleus rotated rigidly. This means that the nucleus behaves rather like a drop of liquid and that the rotation can be pictured as taking place somewhat as in fig. 11, the central region remaining stationary and the external region moving round like a wave.

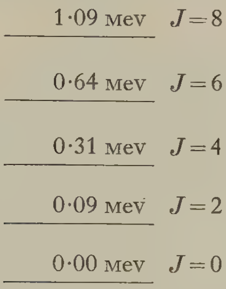

Fig. 10. Rotational energy levels in Hf^{180}.

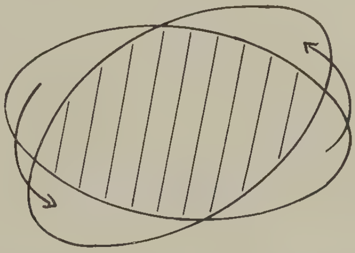

Fig. 11. Qualitative diagram representing the motion of nuclear matter during the rotation of an ellipsoidal nucleus.

Collective motion of nuclear matter can also be obtained in regions where Q is not so large in the form of harmonic vibrations about the equilibrium shape. This sort of motion is characterized by a series of equally spaced energy levels, the spacing being $h\nu$ where ν is the natural period of oscillation of the nucleus. Again it is satisfying that energy levels of this type have been observed for a number of nuclei. In fact, there appears to be an abrupt transition at $N=88$ in that for nuclei with $N > 88$ the characteristic level spacing for rotational levels is observed, whilst for $N < 88$ the excited states are vibrational in nature.

These vibrational levels can also be described in terms of the vibration of a drop of liquid, in the same way that rotational levels can be related in some sense to the rotation of a distorted liquid drop. The idea that the nucleus might be compared to a liquid drop is, of course, not new; it was first suggested by Niels Bohr in 1936 in connection with a description of nuclear reactions. A nucleon entering a nucleus and giving up its energy is likened to a heating up of the drop and the subsequent emission of particles in the nuclear reaction is analogous to evaporation. The liquid drop description of the nucleus is also strikingly supported by the fission process in which a nucleus struck by a neutron undergoes such a violent 'shaking' that it breaks up into two large nuclear fragments. This simple description cannot, however, account for any very detailed properties of the nucleus; in such cases reference always has to be made to individual nucleons in the nucleus.

6. CONCLUSIONS

The shell model and the collective model are the two basic models of the nucleus and between them they account in a semi-quantitative fashion for a great many nuclear properties. The obvious question to ask is to what extent the models are equivalent and whether they approach a 'true' description of the nucleus.

The problem of their equivalence has occupied theoretical physicists for a number of years. Recently some success has been achieved in showing that shell model states can on occasions closely resemble the rotational and vibrational states of the collective model and it has even been possible to make approximate calculations, for example, of the moment of inertia or the spacing between vibrational levels.

Another approach to these problems has been to unify the ideas of both models in the so-called 'unified model'. In this treatment is it supposed that the nucleons are moving in an ellipsoidal potential well rather than a spherical one and that the whole can undergo collective rotational motion. The level schemes then become very complicated, but it has nevertheless been found that this description does help to account for a large number of experimental data.

All the models to some extent illuminate the true nature of the nucleus and are at the moment the most suggestive and useful way of describing it. However, there has recently been a number of developments in techniques for the direct mathematical treatment of the formidable problem presented by the many-body nature of the nucleus. Such calculations have as one of their main objectives the prediction of the various parameters that we have already introduced empirically in the various models such as the nuclear size, density distribution, binding energy, potential well depth, etc. Furthermore, the detailed theory must also show the relationship between the true nuclear state and the corresponding model state. Already some results have been obtained on the nuclear size and binding energy and it seems clear that within the next few years the detailed calculations will be improved enormously giving us very considerable information about the actual state of the atomic nucleus.

READING LIST

The following are recommended for further reading:

General

" On the Structure of Atomic Nuclei ", by A. Bohr, *Amer. J. Phys.*, 1957, **25**, 547.

Nuclear Structure, by L. Eisenbud and E. P. Wigner (Princeton: University press; London: Oxford University Press, 1958).

Shell Model

Elementary Theory of Nuclear Shell Structure, by M. G. Mayer and J. H. D. Jensen (New York: John Wiley and Sons, Inc., 1955).

Collective Model

Rotational States of Atomic Nuclei, by A. Bohr (Copenhagen: Ejnar Munskgaard, 1954).

Alexander von Humboldt and the organization of international collaboration in geophysical research

by L. KELLNER
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Summary

The history of research into geomagnetic phenomena in the first half of the nineteenth century is given in outline. An account is given of the rôle played by Alexander von Humboldt in the organization of international collaboration in this field.

A hundred years have passed since the death of the scientist Alexander von Humboldt. In his lifetime, his contributions to geophysics and geography and his very wide knowledge had made his name legendary; it is now very little realized that his work left a lasting impact on science, that " the very schoolboy is familiar with his methods, yet does not know that Humboldt was his teacher ", as Agassiz said in 1869 in his centenary address to the Boston Society of Natural History. The centenary of his death, which occurs so shortly after the International Geophysical Year, seems the appropriate moment to recall the rôle he played in the first organization of international collaboration in the field of geophysical research.

Born in Berlin in 1769, into a family of Prussian officers and administrators and Huguenot manufacturers, educated in a period of pedagogical reform which sought to develop a full and harmonious personality, he had been destined by his mother for an administrative post in his country. During his student years he discovered his scientific inclinations; after attending courses in physics and physiology in Göttingen, and after a year's training at the mining academy of Freiberg in Saxony, he entered the department of mining of the Prussian civil service, but resigned from his post at the death of his mother in order to devote himself entirely to his scientific interests. His inheritance enabled him to satisfy his secret longing and finance an expedition into the Spanish colonies in South America, from which he returned, after five years of travel, with a vast collection of plants and of geographical, meteorological and magnetic observations. With these magnetic measurements begins the history of modern geomagnetism and international collaboration in this field.

Stimulated by his work on the intensity of the magnetic field of the earth, he turned to a systematic investigation of the diurnal variations of the magnetic declination immediately after his return to Berlin in 1806. In a summer-house in the middle of a large garden, he set up a magnetometer, the so-called Prony telescope. This rather cumbersome instrument consisted of a magnetic bar, firmly attached to a telescope. The whole was suspended from a torsion head by means of a torsionless suspension, then known as a Coulomb suspension, and enclosed in a glass case. The telescope could be rotated round the bar and used in a position above or vertically below the magnet; it was focused on a distant scale and the angle of deflection determined from the position of the cross-wires. A ' distant scale ' had a very literal meaning at that time, it was fixed to a garden wall, 400 yards away, and illuminated at night with a lantern. The gravest disadvantage of the instrument was the necessity to approach the magnet for every reading. Here, Humboldt carried out about 6000 observations from May 1806 until June 1807, in particular at the times of the solstices and equinoxes. The measurements were taken at intervals of thirty minutes, for several days and nights in succession; at one time, he did not leave his post for a whole week. The diurnal variation of the compass had been discovered by George Graham in 1722 but his observations had not been continued into the night. Celsius' and Hjörter's discovery of violent perturbations during the occurrence of an aurora were confirmed by Humboldt and his assistant Oltmanns in December 1806, and from then on he gave his particular attention to these magnetic storms as they are now called. He soon became

Fig. 1. Alexander von Humboldt in 1805.

convinced that simultaneous observations at a number of distant stations would be needed before it would be possible to discover the causes of the regular daily variations of the magnetic declination as well as the irregular disturbances. His plans for the organization of such stations to the east and west of Berlin were, however, frustrated by the political events; Prussia was occupied by French troops after the defeat at Jena, and Humboldt was ordered to accompany Prince William to Paris in 1807 for negotiations with Napoleon over the French demands for reparations.

During his long stay in Paris, which lasted until 1826, he did not himself continue with his magnetic observations but he succeeded in awakening the interest of Arago with whom he had formed a very close friendship, with the result that regular observations of the magnetic declination were begun at the Paris observatory in 1818 with Gambey's instrument, which did away with the cumbersome telescope. The magnetized bar of iron carried ivory scales attached to the end faces which could be observed through travelling microscopes provided with cross-wires. Thus, it was possible to read either the scale division on the cross-wire or to displace the microscope until the zero of the scale had been brought back into view; in this case, the displacement of the microscope was determined on the divided scale fixed to its stand. A similar instrument had been installed in Kazan on the Volga by the physicist Kupffer, and a comparison of the records of the two stations proved the simultaneity of magnetic storms. Although this phenomenon did not attract general attention among scientists at this time, Humboldt became more determined than ever to continue his work and to urge its importance on others.

It was therefore his first care, on his return to Berlin in 1826, to set up a new magnetic station, this time in the garden of the banker Abraham Mendelssohn-Bartholdy, the father of the composer. A specially constructed, non-magnetic hut was erected in which all metal parts were made of copper; it was draught-proof and kept at a fairly constant temperature and a high one at that; ever since his days in the tropics, Humboldt had liked to maintain an indoor temperature of about 72°. Regular observations began in the autumn of 1828 with a Gambey magnetometer; they were timed to synchronize with those of Paris and with measurements carried out 35 fathoms underground in a mine in Freiberg. Humboldt took his turn at the instrument together with a number of assistants, among whom were men like the astronomer Encke, the mathematician Dirichlet and the physicist Poggendorf, the inventor of the mirror suspension. The collaboration between Berlin and Freiberg soon proved that the diurnal variations in the mine agreed with those obtained above ground.

In 1829 Humboldt made an expedition, at the invitation of the Russian government, to the Urals and Siberia; on his return, he was invited to address a special meeting of the Russian academy in St. Petersburg, a meeting which gave him the right platform for advocating a plan for the organization of magnetic observations throughout the vast Russian empire. He suggested the organization of a network of stations in the European and Asiatic parts of Russia for the purpose of meteorological and magnetic measurements which would be of material as well as scientific benefit, with the academy as the centre. It was his proposal that these observatories were to stretch from St. Petersburg to Kyakhta on the Mongolian frontier and that continuous observations of the magnetic declination, inclination and intensity should be carried out. The hourly meteorological measurements should include barometric readings, determinations of the atmospheric temperature and humidity, the amount of rainfall and the direction of the winds, all of them of the greatest importance for agriculture and communications. The necessary standardization of instruments should be the task of a special institution.

This address was immediately successful; a committee was set up under the leadership of Count Cancrin, the finance minister of the country, and with Kupffer as scientific adviser. The realization of this fairly ambitious plan followed at an astonishing speed. Only a year later magnetic observatories had been installed in St. Petersburg and Kazan, and standardized instruments were in the hands of trained observers in Moscow, Nikolayev, Sitka off the Alaskan coast, Barnaul and Nerchinsk in Siberia; there was even a station in the garden of the Greek monks in Peking.

As Humboldt himself has pointed out (1843), he had been following in the footsteps of Leibniz who had made similar proposals to Peter the Great in 1712, ideas which have been preserved in a letter to the Czar which Humboldt found in the archives at Hanover, and which reads:

> " If it were made compulsory to furnish new observations every ten years, and if new (magnetic) globes were constructed with these results and employed in the practice of navigation, a quantity would, without doubt, be obtained which, for every interval of ten years, could serve for longitudes. If this work were several times renewed, one would in the end succeed in discovering a fixed and permanent point of departure. Now, since the magnet has not only a declination in the horizontal plane but also an inclination in the vertical plane, it is of the utmost necessity to observe the latter carefully. I have built a special apparatus for this purpose (*instrumentum inclinationis*), and I maintain that it would be desirable that the declination and inclination should be observed at many localities and at different periods in the vast empire of Your Majesty which observations would be of the greatest usefulness for the navy."

The year 1832 is a landmark in the history of geomagnetism. Gauss (1833) published his paper on the reduction of magnetic intensity to absolute units, i.e. to the measurement of length and time, of which an English translation of the original Latin appeared in 1833. The Gauss magnetometer, with certain refinements and modifications used to the present day, consisted of a prismatic bar of strongly magnetized soft iron, one foot in length and weighing nearly a pound; in later instruments he increased the weight and size to twenty-five pounds. A skein of thirty-two threads of raw silk, fixed to a torsion head with a divided circle, formed the suspension of the magnet which was provided with a plane mirror on one of the end faces. Two screws permitted the adjustment of the mirror at right angles to the axis of the bar, and the magnet could be turned over in its cradle to correct for an error in collimation. It was enclosed in a wooden box with a hole in the top for the suspension and a hole at the side to let the light enter. Gauss admitted that it would have been advisable to fit this opening with a piece of plate glass but had been unable to procure one of the necessary optical properties. A theodolite was placed opposite the mirror, at a distance of seventeen feet and with its vertical axis in the same meridian plane as the suspension. A four-foot scale, divided into millimetres, was attached to the telescope stand in a plane normal to the magnetic meridian, and the zero point marked by a fine gold thread from which a small weight depended. The fixed magnet which in the original instrument weighed 400 g, was placed on to a wooden arm attached to the telescope stand. The moment of inertia of the rotating magnet was determined by the now classical method of clamping a bar of non-magnetic material and known dimensions and weight to it and measuring the period of oscillation of the combined system and of the magnet alone; the latter performed one full swing in fourteen seconds. The experimental error did not exceed one-tenth of a second. One division of the scale corresponded to an angle of twenty-two seconds of arc. Since Gauss considered it easy to estimate one-tenth of a millimetre, as indeed it is with a little practice, the angle of deflection was known to an accuracy of ±2 sec of arc. The measurement of a magnetic field, or rather of its horizontal component, had now been reduced to the determination of the distance between the centres of the fixed and moving magnets and the time taken for a full oscillation as well as the weighing of the auxiliary bar; the units chosen were the millimetre, milligram and second. The Gauss magnetometer was more sensitive than the older instruments and had the added advantage that the observer kept at a distance from the magnets, avoiding draughts and small temperature changes. In the original paper the author also indicates the possibility of converting the rotating magnetic system into a galvanometer by winding a coil of wire round the magnet. An absolute unit had now been introduced for the intensity of the magnetic field but the general acceptance of it was slow in coming. As late as 1845 it was still customary to express magnetic intensities by reference to Humboldt's early work.

A number of European stations were soon equipped with the new instrument, and simultaneous observations at prearranged periods began in 1834, in Göttingen, Copenhagen, Altona, Brunswick, Leipzig, Berlin, Milan and Rome which formed the so-called Göttingen Association. They first adhered to a plan, proposed by Humboldt in 1830 who was a glutton for work; continuous observations were carried out at intervals not exceeding one hour and for forty-four consecutive hours at the solstices and equinoxes and on the 4th and 5th of May, the 6th and 7th of August and the 5th and 6th of November. The Göttingen Association went over to twenty-four hour readings at intervals of five to ten minutes on the last Saturdays of the longest months.

With a network of stations widely spread over Europe and northern Asia, and a few scattered permanent observation posts in the north, in Iceland and Greenland, and in Mexico and Chile, Humboldt (1836) now tried to interest English science in the organization of similar establishments. So far, only Greenwich and Dublin had followed the procedure of the Göttingen Association. He therefore addressed an open letter to the Duke of Sussex who was then president of the Royal Society and who, in 1790, had been a fellow student of Humboldt's in Göttingen.

> " The generous interest taken by Your Royal Highness in the advancement of human knowledge, encourages me to hope for the favourable reception of the request which with respectful confidence, I now venture to address to you. I take the liberty of soliciting your attention to the labours requisite for the investigation, by precise means, almost constantly employed, of the variations of *terrestrial magnetism*. By obtaining the cooperation of a great number of zealous observers, provided with instruments of similar construction, M. Arago, Mr. Kupffer and myself have succeeded in the last eight years in extending these researches over a very considerable part of the northern hemisphere. Permanent *magnetic stations* being now established from Paris to China, following towards the east from 40° to 60°, I feel myself justified in soliciting, through the intervention of Your Royal

Highness, the powerful cooperation of the Royal Society of London, to sanction this enterprise, and also to promote its success by the establishment of new stations, as well in the vicinity of the magnetic equator as in the temperate part of the southern hemisphere.

" An object which is equally important whether it be considered in connexion with the physics of the earth or the improvement of nautical science, has a double claim upon the attention of a Society, which has from its commencement with constantly increasing success, cultivated the vast field of the exact sciences. Our information respecting the progressive development of the knowledge which we possess of *terrestrial magnetism* must be indeed imperfect, if we are ignorant of the numerous valuable observations which have been made at different epochs, and are still being made, in the British Isles, and in various parts of the equinoctial zone subject to the same empire. Our present object is to render these observations more useful, that is, better adapted to manifest great physical laws, by coordinating them according to a uniform plan, and connecting them with the observations now in progress upon the continent of Europe and northern Asia. . . .

" That which characterizes our epoch, at a time distinguished by grand discoveries in optics, electricity and magnetism, is the possibility of connecting phenomena by the generalization of empirical laws, and the mutual aid afforded by sciences which had long remained isolated. At the present day simple observations upon horary declination or magnetic intensity made simultaneously in situations very distant from each other, reveal, so to speak, what passes at profound depths in the interior of our planet, and in the superior regions of the atmosphere. The luminous emanations, the polar explosions which accompany the magnetic storms, appear to follow great changes in the habitual or mean tension of terrestrial magnetism.

" It would greatly tend to promote the advancement of the mathematical and physical sciences if, under the Presidency and auspices of Your Royal Highness, the Royal Society of London, to which I make it my boast to have belonged for twenty years, would exert its powerful influence to extend the *line of simultaneous observations*, and to establish *permanent magnetic stations*, either in the region of the tropics, on each side of the magnetic equator, the proximity of which necessarily diminishes the amplitude of the horary declinations, or in the high latitudes of the southern hemisphere and in Canada. I venture to propose this latter point, because observations upon horary declination made in the vast extent of the United States are still very rare. Those, however, of Salem, in 1810, calculated by Mr. Bowditch, and compared by Arago with the observations of Cassini, Gilpin, and Beaufoy, merit great praise, and might serve as a guide to observers in Canada in investigating whether the declination there does not diminish between the vernal equinox and the summer solstice, contrary to what occurs in western Europe. In a memoir that I published 5 years ago, I suggested as magnetic stations extremely favourable to the progress of our knowledge, New Holland, Ceylon, the Mauritius, the Cape of Good Hope (rendered illustrious by the labours of Sir John Herschel), St. Helena, and some point on the eastern coast of America to the south of Quebec. . . .

" To promote the rapid advancement of the theory of terrestrial magnetism, or at least to establish with more precision empirical laws, it is necessary at the same time to prolong and to vary the lines of *corresponding observations*, also to distinguish in observations of horary variations what arises from the influence of the seasons, of serene and cloudy weather and of abundant rains, of the hours of day and night, and of the true time at each place, that is, from the influence of the sun, and of all isochronous influences at the different meridians. To these observations of horary declination must be united those of the annual movement of the *absolute declination*, of the *inclination of the needle*, and of the *intensity of the magnetic forces*, the increase of which from the magnetic equator to the poles is unequal in the Western American and the Eastern Asiatic hemisphere. All these *data*, indispensable bases for the future theory, can only acquire certainty and importance by the means of establishments which shall remain permanent for a great number of years, of Physical observatories in which the investigation of numerical elements may be repeated at settled intervals in time and with similar instruments. . . .

" It must be considered fortunate if the isolated labours of travellers whose cause it is my office to plead, have contributed to give animation to a species of investigation which is the work of centuries, and which requires the concurrence of numerous observers, distributed according to a plan arranged after mature consideration, under the direction of several of the great scientific centres of Europe. The directors should not always confine

> themselves to the narrow limits of the same instructions, but they should vary them freely in adaptation to the progressive state of physical science, and the improvement of instruments and methods of observation.
>
> " When soliciting Your Royal Highness to condescend to communicate this letter to the illustrious Society over which you preside, it is not in any degree my office to inquire which are the magnetic stations that merit preference at the present time, or that local circumstances may admit of establishing. To have solicited the concurrence of the Royal Society of London will be sufficient to give new life to a useful enterprise in which I have been engaged for very many years. I venture simply to express the wish that, should my proposition be received with indulgence, the Royal Society would enter into direct communication with the Royal Society of Göttingen, the Royal Institute of France and the Imperial Academy of Russia, in order to adopt measures the best adapted for the combination of what it may be proposed to establish with what already exists upon a very considerable extent of surface."

A committee of the Royal Society (1836) under the chairmanship of the Astronomer Royal, G. B. Airy, warmly recommended the adoption of Humboldt's suggestions, approving on the whole his choice of localities although they voiced a warning that the volcanic rock of St. Helena might have a disturbing effect on the instruments, a warning which was proved by the subsequent measurements to have been well justified. Quebec was rejected for a similar reason; the houses in this town were all roofed with sheets of corrugated iron. A special committee was entrusted with the selection of the most suitable instruments. The delicate question of cost had necessarily to be considered; it was obvious from the scale of the undertaking that it was beyond the means of even the best endowed scientific society and an appeal to the government was advocated, as well as to the munificence of the East India Company.

> " Having very fully laid before the Council the contents of M. de Humboldt's letter, we have now to offer an opinion upon the subject it embraces. There can, we consider, be no question of the importance of the plan of observation which is here proposed for the investigation of the phenomena of terrestrial magnetism, or of the prospect which such a plan holds out of the ultimate discovery of the laws by which such phenomena are governed. . . .
>
> " There are, however, other grounds on which such a proposition as that made by M. de Humboldt should be most cordially received by the Royal Society. This society is here called upon, as a member of a great confederation, to cooperate with several other members, already in active cooperation for the attainment of an object which ought to be common to all, and to such a call the Royal Society can never be deaf. Those who know best what has been done by cooperation on a well-digested system, and what remains undone in many departments of science for the want of it, can best appreciate the benefits that would accrue to science, by the adoption of the extensive plan of cooperation advocated by M. de Humboldt. Independently of our acquiring a knowledge of the laws which govern the phenomena here proposed to be observed, we ought to look to the effect which the adoption of such a plan may have on other branches of science. . . . We might thus hope to see the united efforts of all the scientific societies in Europe directed to the prosecution of inquiry in each department of science, according to the plan of cooperation best adapted for its development."

In the following year the British Association joined forces with the Royal Society; in addition to the establishment of permanent magnetic observatories for a limited time at the localities proposed by Humboldt which had been chosen for their proximity to particularly interesting singularities of the magnetic parameters, they urged the government to equip a naval expedition to the Antarctic seas. The combined pleas met with the greatest success. The East

India Company responded magnificently by setting up magnetic observatories at Madras, Simla, Singapore and Bombay while the Rajah of Travancore provided one at Trevandrum and the King of Oude at Lucknow. The two ships ' Erebus ' and ' Terror ' under the command of Sir James Clark Ross sailed on 30 September 1839 with twenty-four officers and a hundred and four men; after unloading the equipment for the permanent magnetic stations at the island of St. Helena and the Cape of Good Hope, they sailed in the southern hemisphere for three years, moving magnetic observatories. The assistant surgeon of the ' Erebus ', then known as " the son of the famous botanist Sir James Hooker ", had joined the expedition for the opportunity of studying the vegetation in these southern latitudes; Sir Joseph Hooker's ' Antarctic Flora ' began the career of one of the greatest and also most long-lived English botanists. Warm clothing and the respectable amount of twenty-six tons and ten hundredweights of meat and vegetables, all tinned, not to mention nearly three thousand pounds of cranberries, termed ' medical comforts ', ensured that, with the exception of a boatswain who was swept overboard, all came back in a good state of health after three winters in the Antarctic.

The Royal Society (1840) had issued exhaustive instructions to Ross, whose scientific part was based on a letter by Humboldt to the Earl of Minto, First Lord of the Admiralty. Hourly observations were to be carried out on board ship and on land of the horizontal and vertical magnetic intensity and of the absolute declination; it is then possible to deduce the inclination. On the internationally agreed term days, these intervals were to be shortened to two and a half minutes; these investigations of the irregular magnetic disturbances had now been chosen according to the recommendations of Gauss to cover a twenty-four hour period from 10 p.m. Göttingen mean time on the Friday preceding the last Saturday in February, May, August and November. On eight additional days on the Wednesdays nearest to the 21st day of the remaining months, the declination alone was to be observed at intervals of five minutes. Measurements of the barometric pressure, air temperature and atmospheric electricity were a necessary concomitant. In addition, there was the search for the Antarctic magnetic pole and the survey of any land to be seen, but Ross was warned that " no scientific datum of this description, nor any attempt to attain very high southern latitudes, can be deemed important enough to be made a ground for exposing to *extraordinary* risk the lives of brave and valuable men ". Gravity measurements, the temperature of the ocean and due attention to the geology, botany and zoology on land provided a very full programme for the three years. Every contingency had been considered, even the outbreak of war (Ross 1847): " In the event of England being involved in hostilities with any other power during your absence, you are clearly to understand that you are not to commit any hostile act whatever; the expedition under your command being fitted out for the sole purpose of scientific discoveries, and it being the established practice of all civilised nations to consider vessels so employed as exempt from the operations of war." In this century of total wars, such civilized habits are no longer tolerated.

In the final choice, Gambey's instrument had been abandoned in favour of a version of the Gauss magnetometers, designed by Lloyd the director of the magnetic observatory in Dublin; in this way, agreement of experimental methods with the Göttingen Association was preserved, whose membership had greatly increased in the meantime. Every one of the British stations was provided with a dip circle, a transit telescope with an azimuth circle for the necessary astronomical determinations of the true north, two chronometers and a declination instrument, a horizontal force magnetometer and a vertical force magnetometer. The declination and horizontal intensity instruments, the latter a bifilar magnetometer, followed very closely Gauss' design but were completely enclosed for protection from draughts. The vertical intensity was measured with a magnetic bar which rested, by means of knife edges, on agate planes; it was maintained in the horizontal position by weights and the deviations from the mean position were read off by micrometers near the two ends of the bar. The report of the Royal Society Committee provided precise instructions for the setting up and adjustments of the instruments and the proper way of keeping the log-books. Time was to be measured in seconds, mass in grains and velocity in foot per second. At the onset of an aurora, the observers were advised to change immediately from the hourly determinations of the declination to those fixed for the term days.

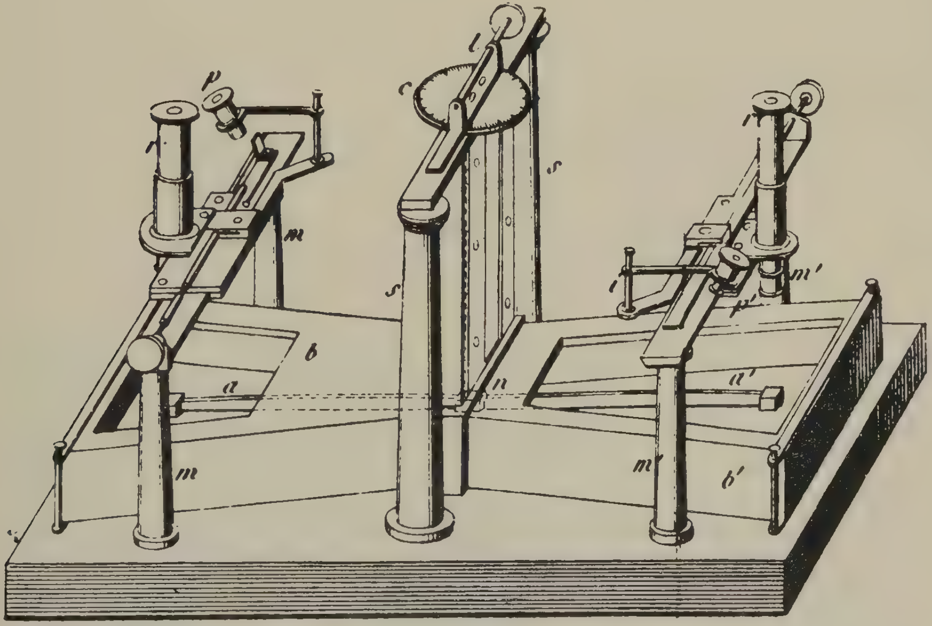

Fig. 2. Prony telescope (modified version by Gambey).

zz′, marble support.
ss, pillars carrying support for suspension.
mm, supports for first microscope.
m′m′, supports for second microscope.
bb′, wooden housing for magnet.
aa′, bar magnet supported by copper ring *n*.
The suspension passes over *l* and is enclosed between glass plates.
rr, microscopes.
pp, magnifying glasses, carried by pins *i*.
Ivory scales are fixed to the ends of *aa*.
The openings in the box *bb* are protected by sliding glass panels.

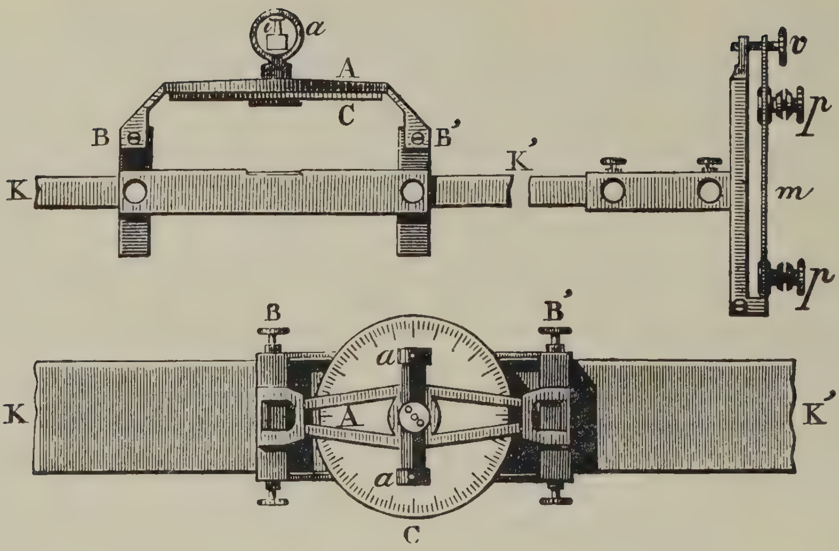

Fig. 3. Unifilar magnetometer after Gauss and Weber.

KK′, bar magnet.
m, mirror.
v, adjusting screws.
pp, screws holding scale.
BB′, stirrup carrying magnet.
AC, plate with divided circle.
aa′, rings resting on knife edges *i* attached to brass bar; this bar is suspended from a silk fibre.

The interest stimulated in scientific English circles by the organization of this forerunner of the international geophysical years had another consequence; at the Glasgow meeting of the British Association in 1840 it was proposed to convene a European scientific congress and to invite Humboldt to act as president: " We are sure that scientific men of all nations would gladly unite in offering this homage to a man whose life and fortune have been spent in their cause, whose voice has been so instrumental in awakening Europe to the inquiry into the laws of terrestrial magnetism, and whose ardent search after nature's truths has triumphed over the Andes and Altai ". The conference was duly convened but Humboldt, engrossed with the publication of his great work *Cosmos*, was unable to attend. In a letter to Arago (1908) of the 31st of December 1840, he explained his reasons in a somewhat disgruntled manner:

> " Occupied and distracted as I am, I have refused the honour offered to me of presiding, on the 18th of September 1842, over the so-called European conference which Mr. Murchison (the well-known geologist, later Sir Roderick Murchison) has invented for Frankfurt, a town which, he says, is like Delphi the navel of the world. To invite a man of 71 years for the 18th of September 1842 ! Over and above that, I should have to compose an address on the progress of science in modern times. Do I have to do so much work on order ? My refusal may displease but I like my liberty more than anything."

An enormous number of geomagnetical data—nearly two million observations were made—was amassed in those three years of international collaboration, providing the material for a greatly increased knowledge of the geomagnetic field and its variations. Even the fairly immediate results settled three outstanding questions, first of them the position of the south magnetic pole, one of the tasks allotted to the Antarctic expedition. Gauss had predicted from his theory of the earth's field that it should be located at 66° southern latitude and 146° eastern longitude, but Ross observed an inclination of 85° 5′ at a position of 65° 15′ S and 144° 53′ E. His measurements of the horizontal intensity finally located the south magnetic pole at 76° S and 154° E, and a recalculation of Gauss' values proved that his first determination had been due to a numerical error.

The results of the ' term-day ' observations marked the beginning of a new chapter in the study of magnetic storms. The simultaneity was strikingly proved by the occurrence, on 25 September 1841 of one of these disturbances

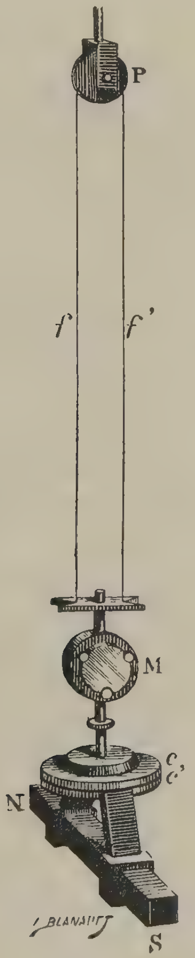

Fig. 4. Bifilar magnetometer after Gauss.
P, pulley.
ff′, steel wire suspension.
M, mirror.
c, fixed divided circle.
c′, rotating divided circle.
NS, bar magnet.

to be recorded at Toronto, the Cape of Good Hope, Prague and in Tasmania, but there only partly since no observations were to be made on a Sunday. This interruption of the regular measurements provoked Humboldt (1845) into the comment: " The English *Sunday observance* which considers it sinful to read a scale and to follow the whole development of great natural phenomena of creation after midnight on Saturdays, interrupted the observation of the magnetic storm since, on account of the difference in longitude, it continued in Van Diemen's Land into Sunday ". The English reviewer of Humboldt's book became equally incensed; he devoted three pages in the *Quarterly Review* (1845–46) to the expressions of his scorn:

> " We are surprised that Baron Humboldt, usually so cautious in imputing blame, should have thus attempted to cast ridicule upon the English Government and English

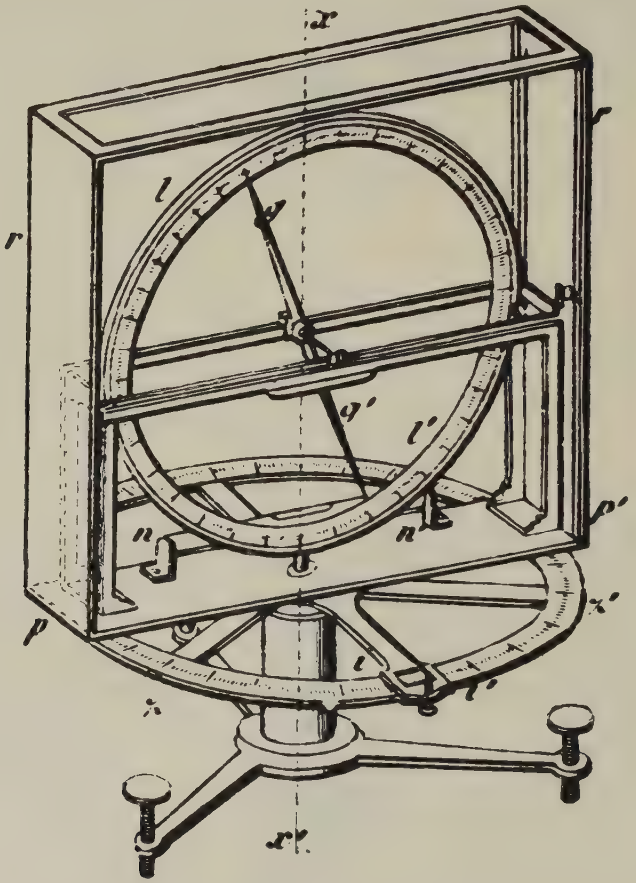

Fig. 5. Inclination magnetometer.

pp', support plate.
rr, glass cage.
gg', magnet, supported by rectangular frame work.
ll', perpendicular divided circle.
xx', vertical axis of rotation.
nn', spirit level.
zz', azimuthal circle.
ii', vernier attached to plate pp'.

men of science, and upon such a ground. . . . Is there, we would ask, an observatory in Europe which has not its *congés de Dimanche* ? . . . Being ourselves fully inclined to regard the usages of different countries and sects with charity, and, indeed, to admit that no absolute standard of conduct can be named on this subject suitable to all nations and at all times, we are surprised that a cosmopolite traveller and grave sage should have on this occasion permitted himself the double indulgence of a blunder and a sneer."

It was left to Colonel (afterwards Sir Edward) Sabine, noted for his explorations and magnetic investigations, to discover the periodicity of magnetic storms and their connection with the occurrence of sunspots. He had been put in charge of the magnetic station of Toronto which, like all the permanent observation posts in the colonies, was manned by officers and non-commissioned officers of the Royal Artillery. In contrast to Sir James Clark Ross, he had been instructed by the Royal Society to restrict the daily observations of the magnetic declination to two-hourly intervals which soon proved to be too widely spaced. In compliance with the spirit rather than the letter of his instructions, he introduced hourly measurements (1851 b).

> " The change from two-hourly to hourly observation was accomplished at the Ordnance observatories by adding with permission of the Master-General of the Ordnance, one non-commissioned officer to the previous strength of the establishment. The extra pay of this artillery soldier for the particular service on which he was employed, was fifteen pence a day, or £22, 16s, 3d a year. This is mentioned because it is not generally known at how small an expense, services of this nature within the competency of the military force stationed in the colonies, can be rendered, whilst the men who are so employed are available for military duty at any moment that may be required."

The observations in Toronto and Hobart in the years 1843 to 1845 had already provided Sabine (1851 a, b) with evidence that the occurrence of magnetic storms showed some periodicity, evidence which was strengthened by the further results obtained in the subsequent three years. When Humboldt (1850) published in the third volume of *Cosmos* an account of the sunspot periods, discovered by the amateur astronomer Schwabe, Sabine (1852 a) was immediately struck with the close agreement between the occurrence of the two phenomena.

> " In our present ignorance of the physical agency by which the periodical magnetic variations are produced, the possibility of the discovery of some cosmical connection which may throw light on a subject as yet so obscure should not be altogether overlooked. As the sun must be recognized as at least the *primary* source of all magnetic variations which conform to a law of local hours, it seems not unreasonable that in the case of other variations also, whether of irregular occurrence or of longer period, we should also look in the first instance to any periodical variation by which we may learn that the sun is affected, to see whether any coincidence of period or epoch is traceable. Now the facts of the solar spots, as they have been recently made known to us by the assiduous and systematic labours of Schwabe, present us with phenomena which appear to indicate the existence of some periodical affection of an outer envelope, or photosphere, of the sun; and it is certainly a most striking coincidence that the period, and the epoch of maxima and minima, which Mr. Schwabe has assigned to the variation of the solar spots, are absolutely identical with those which have been here assigned to the magnetic variations."

The now generally accepted term ' magnetic storms ', denoting strong perturbations of the geomagnetic field, is an invention of Humboldt to whom we owe a number of such new expressions. It appears for the first time in a communication to Gilbert's Annalen (1808), and Humboldt continued to use the word in his correspondence; in his letter to the Duke of Sussex (1836), he introduces it in an apologetic manner. Sabine (1852 b) still speaks of larger magnetic disturbances but adds: "as they are more usually termed in Germany the magnetic storms". With the discovery of the general laws of this phenomenon, Humboldt's expressive term passed into general use.

Thus the research on magnetic storms which had so modestly begun in the garden of a rich brandy distiller in Berlin where Humboldt started his first series of observations, and which at first attracted very little attention in the scientific world, developed into a branch of world-wide investigation through the persistence of one man and the establishment of international collaboration on a previously unknown scale.

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The selection and design of experiments for alternating current teaching

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Summary

In this paper the need for careful selection and design of experiments is stressed. The aims and other aspects of practical class teaching are examined and a method of appraising the educational content of a class as a whole is described with particular reference to alternating current experiments. Some general results of such an appraisal are given.

1. Introduction

In the past ten or twenty years there has been a great increase in the rate at which techniques have been devised or adopted by experimental physicists. The demand for increased accuracy, speed, range or ease of measurement or the exploration and exploitation of fundamental physical phenomena have stimulated the development of new types of apparatus which have either supplemented or replaced existing types.

The adoption of new techniques by the experimental physicist often brings about a radical change in outlook which requires a corresponding change in emphasis in the teaching at undergraduate level. For example, the widespread use of digital techniques has made it necessary to increase the amount of teaching devoted to transient electricity. The student needs to think in terms of step functions as well as sinusoidal waveforms and to understand the relationship between them.

This increased and increasing complexity of contemporary physics presents a great challenge to the organizer of a practical class. The number of experiments that a student may reasonably be expected to carry out remains fixed while the range of techniques and aspects of basic physics that should be covered expands continuously. Clearly it is necessary to overhaul the class at frequent intervals to keep pace with the changing situation and to appraise the class as a whole, as well as each experiment individually, if unnecessary duplication from experiment to experiment is to be avoided. Great care in the selection and design of each experiment in a practical class is essential if the maximum educational content of the class as a whole is to be achieved.

2. Aims of a practical class

When selecting and considering the design of experiments, the organizer must constantly bear in mind the purposes of the practical class.

The first and perhaps most obvious aim is to impart to the student a better understanding of basic physical phenomena. The lectures in physics are largely devoted to the theory of these fundamental processes. The theoretical part of a lecture is often supplemented by a demonstration of a phenomenon, or examples are given of its application, but these, like the descriptions given of experiments carried out to verify the theories, do not involve the student personally.

This personal involvement in an experiment is for many students a necessary condition for grasping the full significance of the concepts being taught. A lecturer could draw upon the blackboard in a few seconds the graphs obtained by a student in an hour or two of practical class work but many students need to obtain the results for themselves before they can fully accept them. It is as if the information is placed in a temporary store in the student's brain and the key of practical work is needed to release it for permanent absorption.

The second aim is to equip the student with a knowledge of some of the techniques used in experimental physics. The experimental physicist is primarily concerned with measurement and this type of technique should receive first consideration when a selection of technique for the basis of an experiment is being made. Experiments may for example be designed around a measurement device such as a dynamometer wattmeter, a measurement circuit such as a bridge or a measurement system such as a cathode-ray oscilloscope.

There is, however, an important class of techniques that cannot in themselves be used for measurement but are rather the component techniques from which measurement techniques are constructed. They can nevertheless form the basis of a measurement experiment. There is for example a well-known technique of impedance matching by means of a transformer. An experiment may be designed in which the primary impedance of a transformer which has a resistance across its secondary is determined by an a.c. bridge and related to the turns ratio, thus teaching a measurement and a component technique at the same time.

In addition to techniques involving the use of apparatus, the experimental physicist has devised or adopted techniques for the analysis of the behaviour of the apparatus and for the evaluation of results. The analytical techniques generally involve the use of mathematics and are chosen to suit the particular problem in hand. For example, in analysing the behaviour of circuits containing resistance, capacitance and inductance and energized by sinusoidal a.c., complex algebra or vector diagrams are used while under transient conditions, differential equations or Laplace transforms are applicable.

The evaluation techniques are necessary for the extraction of information from the measurements that have been made and for the assessment and reduction of errors. The plotting of data in a form that should produce a straight line so that deviations may readily be recognized and the determination of a parameter from its slope or intercept are typical examples.

A third and perhaps the most difficult aim is to instil a feeling for the experimental approach as opposed to the theoretical. The situation has often occurred in physics when a phenomenon has been observed experimentally and further experiments have been planned and carried out to determine more precisely the conditions under which it arises so that the explanation or theory may be evolved on a firmer basis. The intellectual enjoyment afforded by this and many similar problems ought in some way to be communicated to the student. This might perhaps be carried out by an experiment which the student himself plans to determine which of two explanations of a particular phenomenon is correct.

The fourth aim is to familiarize the student with laboratory equipment and facilities. In carrying out the preceding aims the student learns a large number of practical details by experience rather than by direct instruction. For example he finds that a 3 in. diameter meter need not be handled with extreme care whereas a sensitive suspended galvanometer is fragile. If a technique calls for a variable resistance, he becomes familiar with the forms it can take. He learns, often at some cost to the laboratory, that he can take 1 amp from the d.c. mains but not from a 120 v h.t. battery. A practical class can hardly fail to achieve this aim and as a result there is a tendency to underrate its importance and to deny it the attention it deserves. It is very important for example to consider carefully the elimination of the fear of apparatus which is the stumbling block of many students.

In carrying out the four aims outlined above, a fifth and very important aim may be fulfilled; the student should acquire a quantitative appreciation of the effects he is observing and of the apparatus he is using. Again, there is often insufficient attention paid to this function of the practical class; it is inevitable that the carrying out of experiments results in the acquisition of knowledge of this kind and in consequence little effort is made to maximize or optimize the quantitative information.

There are two aspects to this problem. The first concerns the magnitudes of the effects that are actually taking place in the experiment that the student is performing. Very often the student is made aware of these magnitudes by the readings on meters. To take a simple example, the student may be measuring an impedance by determining the current flowing for a given applied voltage. The magnitudes of the three parameters become known to the student. If, however, the impedance is being measured in a bridge circuit the information, in this case the source voltage, that would enable the student to determine the magnitudes of the currents and voltages involved, is not always made available.

The second aspect concerns boundary conditions or the limits of measurement to which a technique or apparatus may be taken. When performing an experiment, the student learns by experience that a particular technique is capable of measuring an effect of a certain magnitude under the prevailing laboratory conditions. This knowledge should be supplemented by an appreciation of what the technique cannot do, i.e. where the technique begins to break down. These are after all the practical details. Usually this appreciation cannot be obtained by experiment on the part of the student. Indeed, class experiments are normally designed so that the limitations are not apparent or at any rate so that they are small enough in magnitude to avoid confusing the student. Information of this sort is probably best given by direct instruction and the appropriate place in this case is the manuscript of the experiment. Attention to the quantitative aspects in practical class teaching will make the student more conscious of the need to give careful consideration to the choice of apparatus and technique.

3. TOPICALITY AND CONFIDENCE

There are many factors in addition to the aims outlined above that must be taken into account when appraising the educational content of a practical class.

Two of the main facets to be considered are the topicality of a technique that forms the basis of an experiment and the confidence that the student gains, in the techniques and in using the apparatus, from the results he has obtained.

The question of topicality is of great importance. The techniques used by physicists increase in number year by year; many are of course discarded but the net increase is still very great. There is consequently a strong case to be made for the selection of techniques to form the basis of a classroom experiment, only from those in current and widespread use. These can be used to teach the student as much basic physics (aim 1) as older techniques and have the advantage of making the student more at home and more useful in his first job. If the fundamentals have been taught and acquired properly the student will be able to work out or find out details of lesser-known techniques if he later requires them. Consideration of topicality applies also to the apparatus that the student uses; it is an advantage to have available in the class only that type of equipment which is currently used in physics laboratories.

The factor of confidence is also very important in the practical class. If a student is unable to verify experimentally within reasonable limits an expression that he has derived or which has been given to him by his teachers, he might tend to lose confidence in the apparatus, fundamental concepts, technique, himself or perhaps ultimately his teacher. Experiments should be designed so that in the early stages the student gains confidence. At a later stage, the student will be able to cope with unexpected behaviour of apparatus without undue worry.

4. Alternating current parameters

The aims and the factors of topicality and confidence are common to all physics practical classes. In a class or part of a class devoted to alternating current teaching each experiment must convey one or more aspects of the phenomena associated with alternating currents. In a typical course it can be assumed that an undergraduate has previously acquired a good understanding of frequency, angular frequency and amplitude from instruction on simple harmonic motion and that the relationship between peak amplitude and r.m.s. values of a sinusoidal waveform may be conveyed in a small part of one experiment. The main effort is needed in giving an understanding of the behaviour of circuits containing resistance (R), capacitance (C), self-inductance (L) and mutual inductance (M) to sinusoidal voltages and currents. This involves the parameter of phase (ϕ).

5. Analysis of the experiments

In an existing class a detailed analysis of all the experiments is a prerequisite to the selection and design of new experiments. A convenient method of analysis is to list the factors outlined above, adding any others which are of interest. The items in the list are in effect questions and the answers are given for each experiment by ticks and crosses representing ‘ yes ’ or ‘ no ’. In these examples the more obvious questions such as cost and number of experiments of one type have been omitted.

Frequent difficulty in answering a particular question may be an indication that the question might be better framed or divided into two questions. Occasional difficulty is however of little consequence; it is the pattern that

Experiment No.		1	2	3	4	5	6	7	8	9	10
Experiment		Addition of impedances	Wattmeter	Matching	C.R.O.	Triode characteristics	Triode oscillations	Heidweiler bridge	Owen bridge	Wagner earth	Transformer bridge
A.C.		√	√	√	√	×	√	√	√	√	√
Aims	Basic	√	√	×	√	√	√	√	√	√	√
	Technique	√	√	√	√	√	√	√	√	√	√
	Experimental	×	×	×	×	×	×	×	×	×	×
	Laboratory apparatus	√	√	√	√	√	√	√	√	√	√
	Quantitative	×	×	×	×	×	×	×	×	×	√
Basic	R	√	√	×	√	×	√	√	√	√	√
	C	×	×	×	√	×	√	√	√	√	√
	L	√	√	×	√	×	√	√	√	√	√
	M	√	×	×	×	×	√	√	×	×	√
	ϕ	√	√	×	√	×	√	√	√	√	√
Technique	Device	×	√	√	×	√	×	×	×	×	×
	Circuit	√	×	×	×	×	√	√	√	√	√
	System	×	×	×	√	×	×	×	×	×	×
Analysis	$a+jb$	×	×	×	×	×	×	√	√	×	√
	Vector	√	×	×	√	×	√	√	×	×	√
Evaluation	Slope	√	√	×	√	√	×	×	×	×	×
	Intercept	×	×	×	×	×	√	√	×	×	×
Confidence		√	√	√	×	×	×	√	√	√	√
Topicality		√	√	√	√	√	√	×	×	×	√

An evaluation of ten specimen experiments in terms of their aims.

emerges from the analysis rather than the fine detail that is of importance. From the pattern the organizer may determine if the framework or skeleton of the class is sound. There are many less tangible factors such as the development of initiative, inventiveness, observation and the enquiring spirit, that cannot readily be analysed. These are the flesh and blood with which the skeleton must be clothed.

6. RESULTS OF THE ANALYSIS

By collecting together the details of all the experiments this type of analysis gives a clearer picture of the class as a whole. The aims that are not being carried out satisfactorily or conversely are receiving too much attention are brought to the notice of the organizer and the merits and demerits of a certain type of experiment made more apparent.

In most of the specimen experiments, three of the aims are adequately covered while the remaining two are not. If, however, the quantitative aim had been divided in the analysis into the two aspects previously described a different picture would have been obtained; in general, experiments in which meters are used (1, 2, 3, 5) cover the first aspect satisfactorily. The basic parameters are also taught adequately with the possible exception of mutual inductance.

An interesting situation arises in connection with bridge experiments (7, 8, 9). The bridge technique is essentially that of measuring an impedance in terms of other impedance elements, i.e. independently of the voltage of the source (and sometimes the frequency) and of the detector sensitivity. Often there are several experiments based on different types of bridge in the one class, i.e. there are several circuit variations of one basic technique. These cover basic parameters excellently but it would perhaps be better to teach the same parameters in conjunction with other techniques.

It can also be seen that some bridge circuits lack topicality; the basic bridge technique is topical but the circuit quite often is not. A typical exception is the transformer bridge experiment (10) which has been selected and designed with the quantitative aim and the mutual inductance parameter especially in mind.

If, in general, the bridge circuits give confidence but are not topical the opposite is true of electronic experiments (4, 5, 6). Experiments based on electronic techniques can be very valuable for teaching a.c. but their design, from the point of view of teaching a technique, often leaves something to be desired. It is argued that since a valve in comparison to a resistor or capacitor is an inaccurate device (in the sense that if a certain voltage is applied to the grid the resulting anode current will vary from valve to valve of the same type and over a period of time with a particular valve), there is no point is using close-tolerance resistors in an electronic experiment. In consequence the student rarely obtains an accurate result from an electronic experiment and he tends to lose confidence in electronic techniques or in his ability to analyse them. Clearly the question of accuracy and confidence needs a great deal of attention.

There is another difficulty encountered in including experiments based on electronic techniques. Very often the student has had no lectures on, say, the thermionic valve and it may become necessary to teach this in addition to basic a.c. parameters. Thus for example an experiment (5) not involving a.c. may be a part of the a.c. teaching. This leads on to an experiment (6) in which an electronic technique and the a.c. parameters are taught at the same time. It is also possible to use a.c. in an experiment without teaching a.c. parameters, as for example in an experiment on a transformer (3) in which the a.c. output is measured for a given a.c. input.

7. SELECTION AND DESIGN OF EXPERIMENTS

This type of analysis enables the selection of experiments to be carried out with greater confidence. If an experiment based on a particulat technique is proposed, its likely characteristics may be matched up against the many requirements enumerated and the experiment accepted or rejected. Alternatively if none of the existing experiments meets a particular requirement, efforts can be concentrated on devising one that is suitable. Having selected the experiment in this way a detailed specification may be prepared on which the design can be based.

Skidding friction
The effect of hysteresis losses in tyre tread rubber

by BARBARA E. SABEY

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SUMMARY

A vehicle skids whenever the friction between tyre and road is insufficient to meet the demands of the driver in accelerating, braking, or cornering. In wet weather, when the water film on the road acts as a lubricant, the friction between tyre and road may be greatly reduced. The magnitude of the friction between a tyre and a wet road depends on the physical characteristics of both the road surface and the tyre.

Recent work has shown that when well-lubricated rubber slides over a hard surface, as in the case of a tyre on a wet road, a large part of the frictional resistance may arise from energy losses in the rubber as it is deformed by projections in the hard surface and then recovers. These are the so-called hysteresis losses. Evidence suggests that if the associated practical problems can be solved very worthwhile improvements in skidding friction may be obtained by the use of tyres in which the rubber of the tread has much higher hysteresis losses than the normal tyre tread rubber.

Skidding friction is only one of the many aspects of frictional behaviour which play an important part in our everyday life. Its importance can be appreciated from the number of road accidents which involve a skidding vehicle. Every year some 30 000 skidding accidents occur in which a person is killed or injured, and about half of these take place when the roads are wet. The problem of skidding on wet roads forms one of the items of research at the Traffic and Safety Division of the Road Research Laboratory.

The problem of skidding is basically one of friction between tyre and road (Grime and Giles 1954–5). According to the classical laws of friction, the coefficient of friction is a constant for any given materials and is independent of, for example, the contact area and the speed of sliding. Experience in many fields in recent years has shown that there are many exceptions to these laws, of which skidding friction is a notable example.

Measurements of skidding resistance on wet roads have shown that the coefficient of friction between the tyre and the wet road may vary considerably with the speed of the vehicle. The faster the vehicle travels the less the time of contact between the tyre and the road, in which the lubricating layer of water film between the surfaces can be removed: thus, the coefficient of friction falls as speed increases. The magnitude of the coefficient depends on various characteristics of the road surface; the surface texture of the road is important, both on a macroscopic scale, i.e. whether it is ‘knobbly’ or smooth-looking (fig. 1), and on a microscopic scale, i.e. whether the individual stones or projections in the surface are smooth and polished or have a harsh sandpaper feel. The coefficient also depends on the characteristics of the tyre. Most motorists would claim from experience that they feel safer with good patterned tread tyres than with smooth tyres which have no pattern. Measurements confirm that, on smooth-looking surfaces, the friction between the tyre and the road can be appreciably greater with a patterned tyre than a smooth tyre, because of the drainage effect of the tread pattern. Other factors such as the hardness of the tyre tread are also important in determining the coefficient of friction. It is apparent therefore that the classical laws of friction are not strictly applicable to the problem of skidding on wet roads.

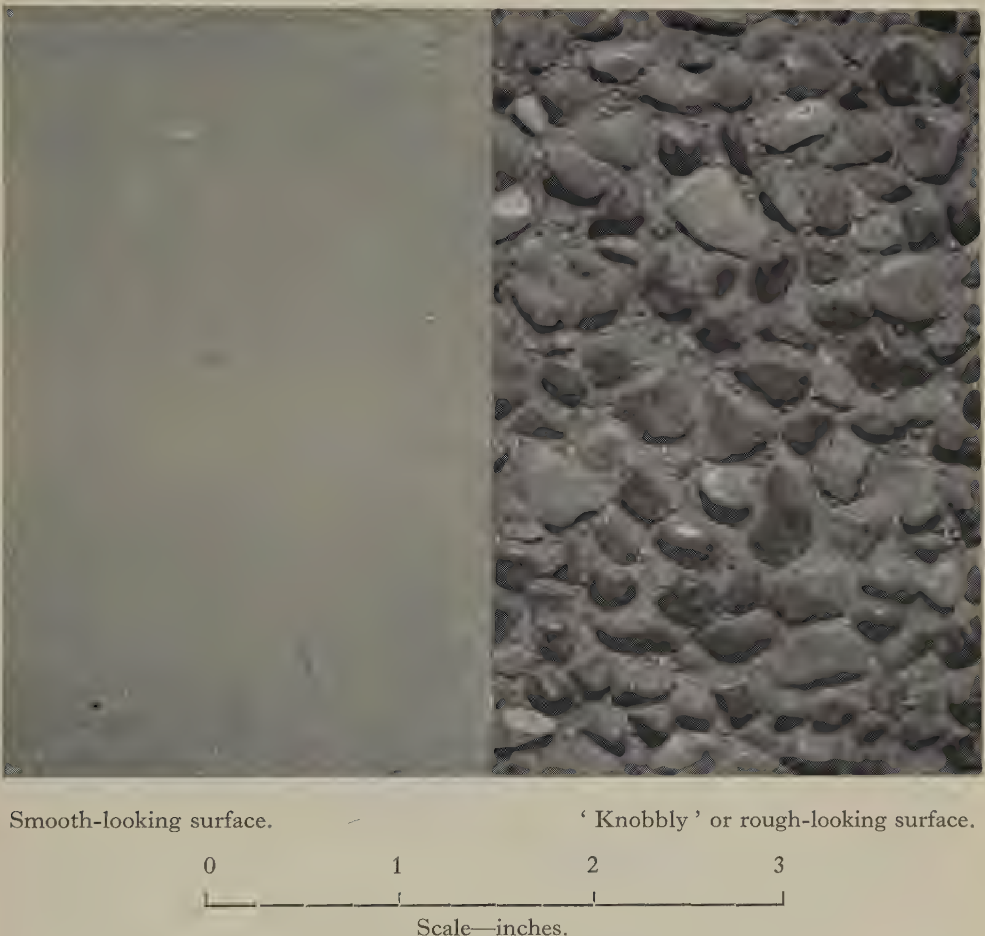

Fig. 1. Appearance of surfaces of different textures.

Under clean, dry conditions the tyre/road combination gives a good skidding resistance, coefficients of friction being of the order of 0·8 or more. On the other hand, when a vehicle skids on a wet road, the water film present between tyre and road acts as a lubricant and the available friction depends on the extent to which this lubricating layer may be removed and areas of dry contact between tyre and road established. Coefficients of friction under these conditions may be as low as 0·1 and are rarely greater than 0·8. On a wet road it is necessary to break through the water film and displace it. To do this it is necessary first to have road surfaces in which the small-scale projections have sharp edges, on which high pressures are built up and assist in breaking through the water; secondly, drainage channels are required to enable the water film to escape, and these may be provided either by having a ' knobbly ' surface on the road or a pattern on the tyre.

The first of these requirements is not always easy to obtain. Many types of roadstone tend to polish under the action of traffic and in some areas of the country, where there is no local stone with a good resistance to polishing, suitable material to provide non-skid surfaces must be hauled over long distances. There are consequently some miles of road where the surface presents a polished finish, the coefficient of friction under wet conditions is of the order of 0·3 to 0·4 at 30 mile/hr and the surface is potentially slippery in wet weather, even at relatively low driving speeds. At high speeds, 70 to 100 mile/hr, the problem of skidding becomes very much more serious. Because the time of contact between tyre and road is so short at these speeds, the expulsion of the lubricating water film is extremely difficult to effect. Even on roads with high coefficients of friction at low speeds (0·6 to 0·7 at 30 mile/hr), the coefficients at over 70 mile/hr may have decreased to very low values.

It has been increasingly apparent over the last few years, as speeds of modern traffic increase, that the road engineer's task to provide roads satisfactorily non-skid in wet weather, is becoming more difficult and costly. At the same time, much consideration has been given to tyre characteristics in an effort to increase friction between tyre and road. Changes in tread pattern design have resulted in such improvements that most modern tread patterns are very effective in providing drainage between tyre and road to assist in removing the water film. But even the most effective of these patterns cannot improve the skidding resistance on rough-looking surfaces where the stones have polished, nor can they provide freedom from skidding at high speeds. Recently, however, work carried out by Dr. Tabor of the Cavendish Laboratory at Cambridge (Greenwood and Tabor 1958), and by the Road Research Laboratory (Sabey 1958) has opened the way to substantial improvements in skidding resistance. The work has shown that hysteresis losses, which play an important part in determining rolling friction, can also play a large part in sliding friction, and may dominate the frictional behaviour when the sliding surfaces are well lubricated (Tabor 1958).

When a hard body rolls or slides over rubber it compresses the rubber in front of it and so does work on the rubber. As the body moves forward the rubber pressing on the rear of the body recovers elastically and urges it forward. If the rubber were ideally elastic the energy restored as the rubber recovered would be exactly equal to the energy stored in the front portion of the region of contact. However, no rubber is ideally elastic. Energy is always lost when rubber is stressed and then unstressed: such losses are the so-called hysteresis losses. There is therefore a resistance to motion determined by the hysteresis losses in the rubber. Rolling friction arises almost entirely from these hysteresis losses; sliding friction is determined only partly by hysteresis losses, and in many circumstances this component is negligible compared with the friction component arising from adhesion between the sliding surfaces. Under well lubricated conditions, however, when the adhesion is small, the hysteresis component of friction will predominate and the higher the hysteresis losses, the higher the frictional resistance.

Such conditions can arise in the case of skidding on wet roads and it has now been shown that when a tyre slides over a wet road surface the energy lost in deforming the tread rubber by dragging the projections in the road surface through it, can make a significant contribution to the resistance to skidding (Giles and Sabey 1958). If the rubber is resilient or ‘ bouncy ’, as are most tread rubbers in use on our vehicles today, the hysteresis losses are small and the hysteresis component of friction correspondingly small; if the rubber is ‘ dead ’, hysteresis losses are high, and the friction correspondingly higher.

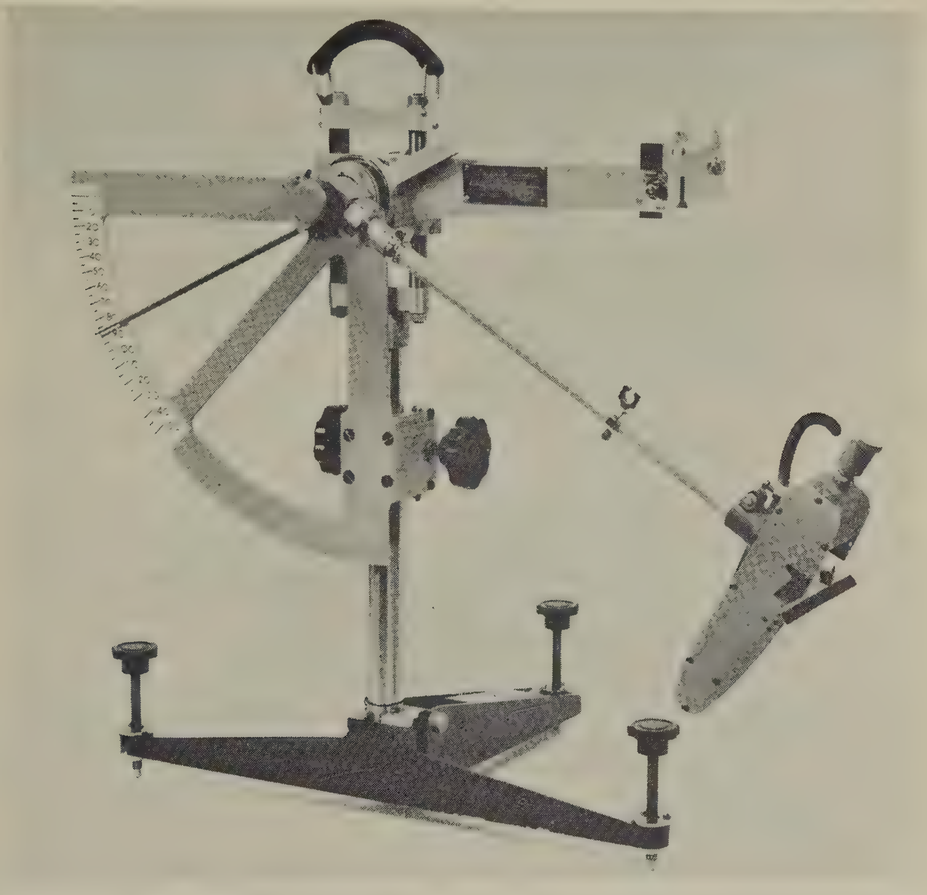

Fig. 2. Apparatus for measuring dynamic friction (scale reads 100 × coefficient of friction).

To investigate how friction changes when using rubbers with different hysteresis losses, tests have been made in the laboratory with a simple apparatus for measuring dynamic friction (fig. 2). In this apparatus a rubber pad is used as the slider; this is mounted on the end of a swinging arm so that one edge slides under a fixed load over a fixed length of test surface, the conditions being such that they give results which have been found to correlate with measurements of friction made when vehicles skid. The frictional resistance between slider and surface is evaluated directly from the loss in energy of the swinging arm, and the instrument is directly calibrated to read the coefficient of friction.

Tests have been made using two rubbers on a number of wet surfaces of different textures: one rubber was of normal tyre tread specification—a bouncy rubber (65 per cent resilience), the other was a rubber of high hysteresis losses, i.e. a ‘ dead ’ rubber (10 per cent resilience). On rough-looking surfaces (see table 1) which have projections in the surface to deform the rubber, the high hysteresis loss rubber produces substantial increases in the coefficient of friction; on smooth surfaces where the rubber undergoes no repeated deformations and the hysteresis component of friction is therefore small, the high hysteresis loss rubber gives similar friction coefficients to the normal tyre tread rubber.

	Coefficient of friction—surface wet	
	Tread rubber specification (65 per cent resilience)	Rubber with high hysteresis loss (10 per cent resilience)
Smooth-looking surfaces		
Glass	0·14	0·13
Smooth concrete	0·23	0·24
Rough-looking surfaces		
Macadam with polished stones	0·32	0·53
Surface dressing	0·57	0·84

Table 1. Friction tests using rubbers with different hysteresis losses on surfaces of different textures.

The importance of these hysteresis loss effects may be illustrated in a striking manner by a simple demonstration due to Dr. Tabor. A rough-looking surface is represented by a sheet of ‘ borealis ’ glass and inclined at an angle of about 10° to the horizontal. Small weighted rubber pads of the same size and shape but having different hysteresis properties are used as the sliders. To obtain conditions of good lubrication the glass surface and surfaces of the rubber sliders are well soaped. When the two sliders are placed at the top of the ‘ rough ’ inclined plane and released, the resilient rubber, which gives less resistance to sliding, travels much more rapidly down the incline than the high hysteresis loss rubber. The difference in resilience is easily demonstrated by bouncing a steel ball on the two rubbers.

Some indication of the kind of increase that may be obtained in practice by using tread rubbers with high hysteresis loss is given in table 2, which summarizes typical results obtained in braking tests carried out on two test surfaces on the Road Research Laboratory's skidding test track. The tests were made by measuring the deceleration of a vehicle when all four wheels were locked for one second at 30 mile/hr. Under these conditions, the retardation of the vehicle is equivalent to g times the coefficient of friction between tyres and road surface (where g is the acceleration due to gravity). With smooth tyres both surfaces give results which indicate that in wet weather skidding difficulties are likely to arise. Because of the effect of the added ‘ drainage ’ they give, patterned tyres give a marked improvement on surface A but none on surface B where ‘ drainage ’ is already good. On the other hand, with the added contribution due to the effects of hysteresis losses when the third set of tyres is used, results are greatly improved on surface B, and some improvement is also obtained on surface A. Thus simply by changing the characteristics of the tyres and making use of the contribution hysteresis effects can make, the results on these two surfaces have been transformed from values that would be associated with quite a high risk of skidding to values that would be considered as representing a high degree of safety.

Test surfaces	Retardations measured with all wheels locked for one second from 30 mile/hr (surfaces wet) using:		
	Smooth tread tyres	Conventional patterned tyres	Patterned tyres with high hysteresis loss rubber
A. Smooth-looking surface	0·35 g	0·63 g	0·78 g
B. Rough-looking surface	0·35 g	0·34 g	0·67 g

Table 2. Results of braking tests on two test surfaces.

In the past, tyre manufacturers have tended to keep hysteresis losses in tread rubber as low as possible in order to reduce power losses in tyres and avoid the risk of overheating and softening of tyres. However, now that the important part hysteresis loss effects play in determining sliding friction under lubricated conditions is realized, the use of high hysteresis loss rubber for the treads of tyres must be considered. If the hysteresis loss effect on skidding resistance can be exploited without leading to serious difficulties in other directions, the way is opened to substantial increases in friction coefficients between tyre and road, and hence greater freedom from skidding in wet weather.

ACKNOWLEDGMENTS

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Light sources using radioisotopes

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SUMMARY

The use of radioisotopes in conjunction with phosphors to produce practical sources of light is described. These sources are completely self-contained, require no maintenance, and provide useful light for several years. The work was carried out at the Atomic Energy Research Establishment, Harwell, Berks.

1. INTRODUCTION

The direct conversion of nuclear energy into a more immediately useful form has obvious advantages. Although not applicable on a large scale one such process has been known for a long time, namely, the conversion of alpha- and beta-particle energy into light by allowing the particles to impinge on a phosphor. This phenomenon is well known in the form of luminous paint, in which a small amount of radium provides the energy and specially activated zinc sulphide crystals are the phosphor. Unfortunately, the zinc sulphide is damaged by the alpha-particles and this limits the useful life of the paint unless the concentration of radium, and hence the luminosity, is kept to a low level. Moreover, radium is dangerously toxic and emits rather a lot of penetrating gamma radiation. This presents a hazard to the manufacturer and personnel exposed to large quantities as, for example, the pilot in a cockpit if many dials are illuminated with radium paint.

When a wider choice of isotopes became available from nuclear reactors a search was made for more suitable radioactive substances which would present less hazard and produce more light. At Harwell, and in America, several practical types of light source have been produced employing isotopes which radiate mainly beta-particles. As beta-particles are much lighter than alpha-particles, they do not damage the phosphor to the same extent, and as they are not so penetrating as gamma rays it is fairly easy to shield the user from the radiation.

2. POSSIBLE ISOTOPES

2.1. *Desirable characteristics* (*table* 1)

Half-life

If the source is to have practical value it should remain luminous for a reasonable time, preferably several years. In a sealed light source the quantity of radioactive material gradually decreases at a steady rate which is fixed for any particular isotope. This rate is expressed as the half-life of the isotope, which is defined as the time required for half the radioisotope to disintegrate. In the case of krypton-85 the half-life is ten years, which means that in twenty years the source will contain one-quarter of its original activity.

Alpha and gamma rays

These should preferably be absent. Alpha rays would damage the phosphor and also make the material dangerous if ingested, while gamma rays limit the amount of activity which can safely be used in a source without heavy shielding.

Daughter products

The isotope should not disintegrate into a daughter product which emits alpha or gamma rays.

Toxicity

It should be non-toxic chemically and radiologically.

Availability

Ready availability and low cost are desirable.

	Half-life (years)	Maximum beta energy (MEV)	Gamma-ray (per cent)	Toxicity	Remarks
Tritium (H^3)	12	0·018	nil	low	Low energy. Ideal for luminous paint
Promethium-147	2·6	0·23	nil	medium	Expensive. Short half-life
Krypton-85	10	0·67	0·5 per cent of 0·5 MEV	low	Inert gas. Low cost
Strontium-90	28	$\begin{cases}0{\cdot}54\\2{\cdot}2\end{cases}$	nil	high	Good except for extreme toxicity
Thallium-204	4·1	0·76	nil	medium	Short half-life. Low specific activity

Table 1. Possible isotopes and their relevant properties.

2.2. *Strontium*-90

This has much in its favour from the point of view of light production, but its great radiological toxicity makes its widespread use unwise.

2.3. *Krypton*-85

Krypton has the great advantage of being a chemically inert gas, which makes it one of the safest isotopes for use in light sources. Decontamination is not necessary in the event of breakage as all the krypton is rapidly dispersed by normal ventilation. It is not retained in the body for any length of time if inhaled. Unfortunately the small gamma emission limits the quantity of krypton-85 which can be employed for a specific purpose. However, the dose rate from small sources is quite low and only short distances with little or no shielding are required to reduce it below existing regulation levels.

2.4. *Tritium* (H^3)

Tritium is also gaseous but can be obtained as solid compounds. The low energy beta-particles from tritium present practically no hazard even when the tritium is enclosed in the thinnest vessel. It is, however, rather more hazardous than krypton if accidentally taken into the body, as it exchanges rapidly with the hydrogen in the body structure. Although its radioactive half-life is twelve years, its ' biological half-life ' is only some twelve days due to loss of hydrogen by natural excretion.

For these reasons work on atomic lights at Harwell has been restricted to krypton-85 and tritium.

3. PHOSPHORS

The role of the phosphor is to convert incident radiation into ultra-violet, visible or infra-red. There are many hundreds of phosphors and only those of the zinc sulphide type will be considered here as they have proved the most efficient for this application.

The mechanism of energy conversion is too complex to be explained fully in this article. Suffice it to say that zinc sulphide, an insulator, can be pictured as having filled shells or energy levels of electrons surrounding the zinc and sulphide ions. Incident radiation of several kinds can raise electrons from the outermost levels into the next unfilled shells, which are then termed excitation or conduction bands. When the electrons return to their former or ground state the excess energy is re-emitted, generally as heat. A small amount of certain impurities in the crystal allows the excited electrons to occupy intermediate energy levels on their way back to the ground state. On passing through these levels part of the excitation energy is emitted as light.

The colour of the light depends on the nature of the foreign atoms. For example, a few parts per million of silver or copper in zinc sulphide produce blue or green emitting phosphors respectively, while varying concentrations of cadmium sulphide in zinc sulphide produce yellow to red light.

4. PRACTICAL LIGHT SOURCES

It would seem that one has only to line a transparent bulb with phosphor and fill it with radioactive gas in order to produce a light source. Most of the beta-particles would hit the phosphor sooner or later, and in fact such a source is quite successful with tritium. As the beta rays from tritium only penetrate a few microns of the phosphor quite a thin layer suffices, and the main problem is the technique of lining the bulb.

With krypton the problem is not so simple due to the higher energy of the radiation. Firstly, a much thicker layer of phosphor (one to two millimetres) is required to absorb all the radiation, and this is opaque to light. Secondly, most common glasses and plastics are darkened by krypton radiation as are also most binder materials used to hold the phosphor in place. The first problem has been overcome in two ways. One, by using a bulb with a critically thick lining of phosphor which will absorb most of the radiation and also transmit a lot of the light produced, and the other by employing a cup-shaped source which allows an excess thickness of phosphor to be used but emits the light through a clear window. In both types special ceria stabilized glass which is resistant to radiation induced coloration has been employed. These sources are illustrated in figs. 1 and 2.

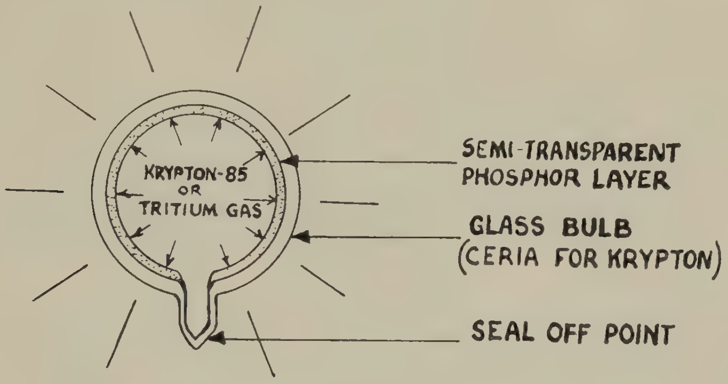
KRYPTON-85
OR
TRITIUM GAS
SEMI-TRANSPARENT PHOSPHOR LAYER
GLASS BULB (CERIA FOR KRYPTON)
SEAL OFF POINT

Fig. 1. Bulb source with critically thick phosphor layer.

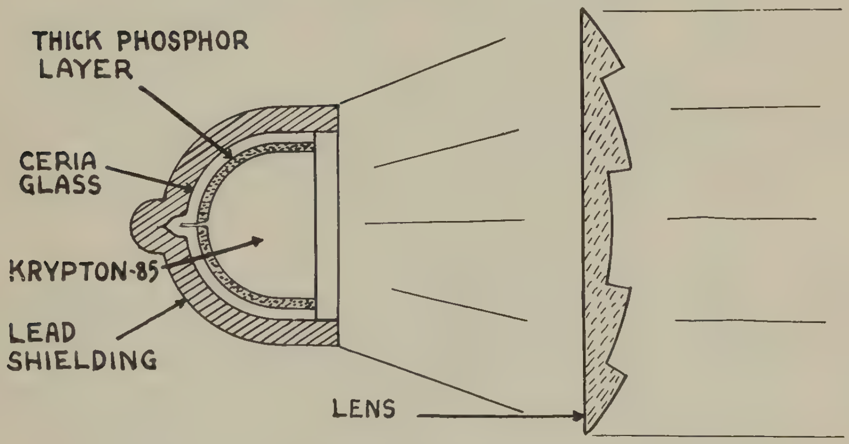

Fig. 2. Cup-shaped source giving directed light beam.

Although plastics such as Perspex suffer damage and slight discoloration to a depth of about 1 mm, this disadvantage is outweighed by the ease of construction and robustness of sources made of these materials. Perspex is not satisfactory for containing tritium gas, however, which fairly quickly exchanges with the combined hydrogen and so is effectively removed from the system when it has penetrated the Perspex to depths greater than the range of the beta-particles.

4.1. *Filling technique*

A simplified form of vacuum system is illustrated in fig. 3. The source is evacuated and then filled with krypton-85 from the stock which is kept in the bulb of activated charcoal. The Toepler pump allows the required quantity to be metered out for any size of source, and by raising the reservoir of mercury the gas can be forced into the source up to one atmosphere pressure. The source is then sealed off and the excess krypton in the pipe-lines collected by cooling the charcoal finger in liquid nitrogen.

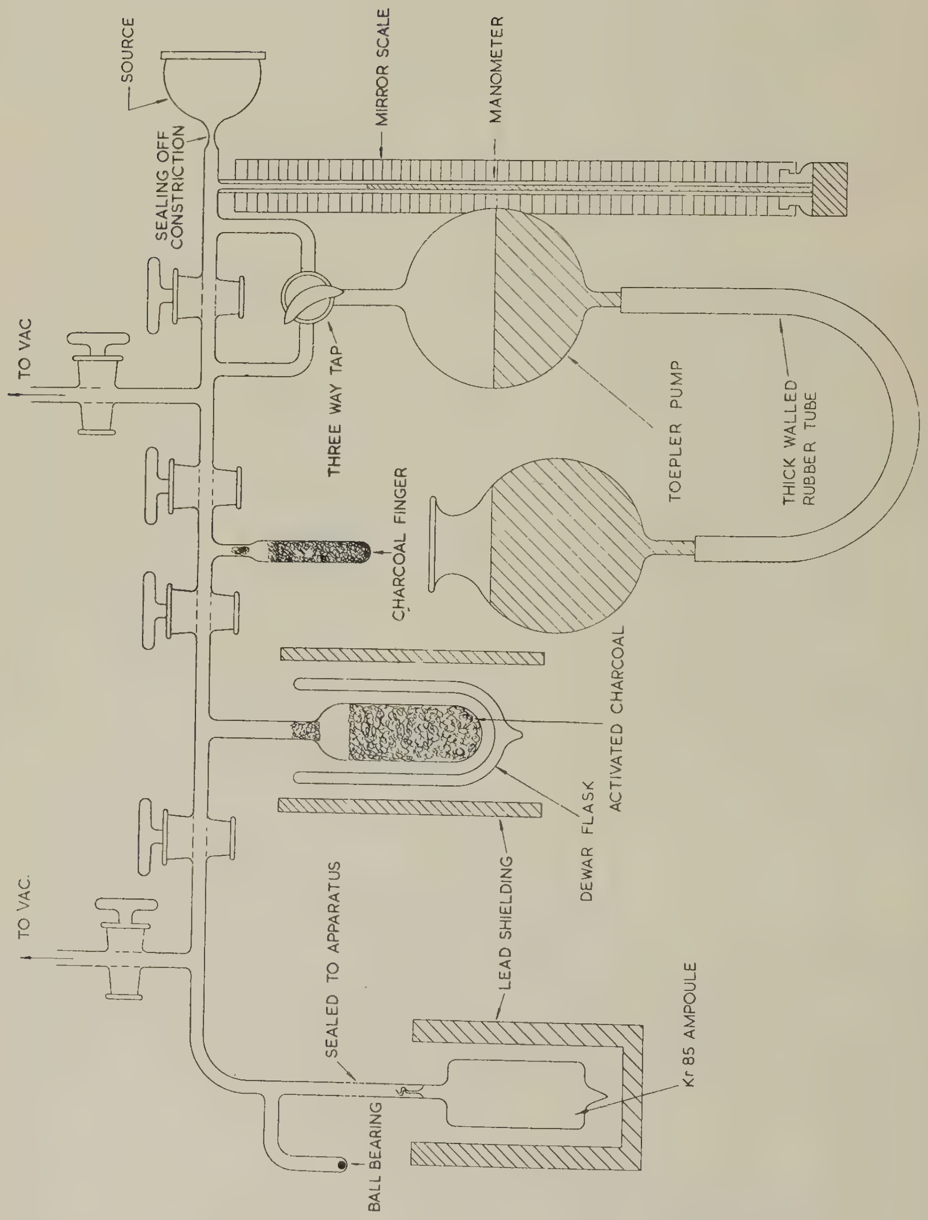

Fig. 3. A simple filling system for krypton-85 sources.

4.2. *Other types of source*

Although the two sources described are the most efficient, other shapes and types have their advantages. For example, letters can be channelled out of a sheet of Perspex, lined with phosphor, and filled with gas. This type has been used in exit signs for the Cockroft Hall at Harwell.

It was hoped to produce brighter sources by incorporating krypton-85 into the remarkable clathrate compound which is formed when quinol is crystallized in the presence of high pressures of krypton. Unfortunately the product proved to be too unstable when radioactive krypton was employed. Work is progressing on the use of solid tritium compounds which should be useful for dial marking and similar applications where radium paint is too hazardous.

5. PERFORMANCE

One curie of krypton-85, or 35 curies of tritium radiate about 2–3 mw of energy. It is obvious, therefore, that considerable amounts of radioactivity would be required to compete with normal means of illumination, especially as the maximum energy to light conversion for zinc sulphide is only some 30 per cent, and the optics of the source reduce this to about 3 per cent. However, the light is all obtained near the required wavelength and wasteful filters, necessary with electric light, are not needed.

A one-curie krypton light source fitted with an eight-inch lens is visible at 1000 yards in darkness and provides sufficient light with which to read newsprint held three or four feet from the lamp. At the lower end of the scale, markers of less than one millicurie are easily visible at several feet.

5.1. *Colours*

Although any spectral colour can be produced by suitable choice of phosphor, the sensitivity of the eye, as shown in fig. 4, means that much more energy, and therefore radioactivity, is required to make a red or blue lamp as bright as a yellow or green one. The graph illustrates the observed intensity for light of the same energy intensity at each wavelength.

5.2. *Useful life*

Although the half-life of the activity is 10–12 years for krypton and tritium the light from sources containing these isotopes tends to fall off rather more rapidly. This is mainly due to radioactive darkening of the constructional materials. The rate of fall tends to lessen after a fairly rapid initial decline in brightness, and at present, results suggest that the sources will be about half their original intensity in three to five years. As time goes on the fall in intensity will progressively tend towards the half-life of the activity.

Sources containing very small amounts of radioisotopes do not show such a rapid decline in brightness.

6. RADIATION HAZARDS

It is obvious that the mere presence of radioactivity will tend to restrict the use of these light sources. The hazards fall into two categories—the gamma radiation from sealed sources, and the beta radiation from a broken source or ingestion of the radioactive substance in solid form.

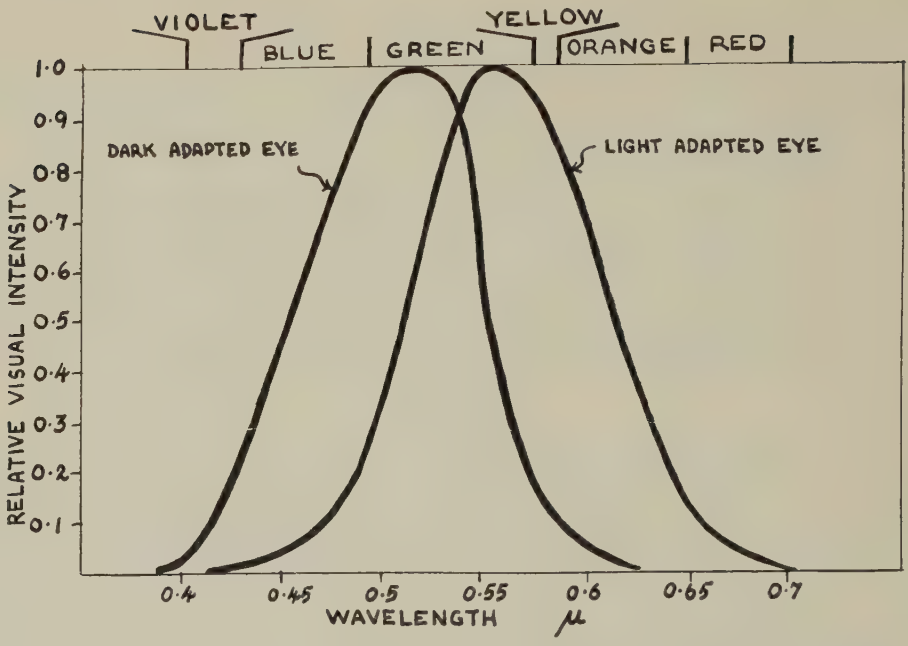

Fig. 4. The ' relative luminosity ' response curves to the equal-energy spectrum, for dark-adapted and light-adapted eyes.

The existing recommendations are as follows. For occupational workers—7·5 milliroentgens per hour (m.r.h.) during a 40 hour week with an overall maximum of 5 roentgens per year. This level to be reduced to one-tenth for the general public. The permissible dose rate on packages for road transport is 120 m.r.h. on the outside of the vehicle with less than 7·5 m.r.h. at the driver's seat. By rail the surface dose should be less than 12·5 m.r.h. and by post less than 10 m.r.h. per 24 hrs. This latter level is low enough to protect unexposed film which may travel with the source.

6.1. *Dose rate from krypton*-85

The gamma dose from krypton-85 comprises two parts. The natural 0·52 MeV gamma and a certain amount of secondary radiation of various energies up to 0·67 MeV which depends on the constructional material in which the beta-particles are stopped.

The dose rate from an unshielded source in a Pyrex bulb thick enough to absorb all the beta radiation is as follows:

Distance	2 cm	5 cm	10 cm	25 cm	50 cm	100 cm
m.r.h.	4·15	0·665	0·166	0·027	0·006	0·002

Table 2. Gamma dose per millicurie of krypton-85.

These results were obtained by extrapolation from direct ionization measurements at 25 cm from a bulb containing 500 mc of krypton-85. Point source geometry is assumed in this table, but practical sources do not behave as point sources at close range and the surface dose should therefore be measured for each shape of source. For example an exit sign had only one-eighth of the extrapolated dose on its surface at the worst point.

6.2. *Shielding*

Due to the low energy secondary radiation, absorption of krypton gamma radiation is not logarithmic with increasing thickness of absorber. This effect is most pronounced up to the first $\frac{1}{16}$ in. lead after which the absorption curve is nearly exponential. Table 3 shows some actual results.

From tables 2 and 3 it can be seen that a combination of shielding and/or distance can reduce the dose rate to a reasonable level for sources up to one curie. If a source is to be mounted in an inaccessible place considerably more activity could be tolerated, but each application must be considered on its own merits.

The secondary gamma radiation from tritium sources is negligible for most purposes.

Thickness of lead shielding	nil	$\frac{1}{16}$ in.	$\frac{1}{8}$ in.	$\frac{1}{4}$ in.	$\frac{3}{8}$ in.	$\frac{1}{2}$ in.
Dose rate as per cent of unshielded dose	100	44·5	35·0	22·7	14·5	9·7

Table 3. Absorption of krypton gamma-radiation.

6.3. *Beta hazard*

For the gaseous isotopes this depends largely on the ventilation. Experiments have shown that with only one window or door open in a laboratory a release of activity is dispersed in a few minutes. The sources are robustly made and failure unlikely, but consideration must be given to this point if sources are to be used in confined spaces such as submarines.

Solid tritiated substances must of course be handled as any other solid radioactive material during manufacture, but its toxicity is very much lower than most active materials.

7. Possible uses

It is likely that these sources will initially be used where they reduce a hazard far greater than the slight hazard introduced by the radiation. This would apply to most types of emergency equipment which needs to be located in case of power failure or in the field. If the use of electricity is undesirable as in petrol refineries, ammunition dumps, sewers or other explosive atmospheres they could be of value. Finally, if other sources of illumination are impracticable as on small moving parts such as switch toggles, or inaccessible places where maintenance is a problem, these sources could be used.

It is stressed that they are of low light output and cannot compete with normal light in this respect, but such is the sensitivity of the human eye that even these low levels of illumination can be exceedingly useful in the absence of normal lighting.

References

British Patent Application No. 7964/58. Cavity light source using krypton-85.
British Patent Application No. 28033/57. Radioactive gas in clathrate compounds.

ESSAY REVIEWS

Turning Points in Physics. A Series of Lectures given at Oxford University in Trinity Term, 1958. By R. J. Blin-Stoyle, D. ter Haar, K. Mendelssohn, G. Temple, F. Waismann, and D. H. Wilkinson, with an introduction by A. C. Crombie. (Amsterdam: North-Holland Publishing Company, 1959.) [Pp. 192.] 21*s*.

Just before the outbreak of the 1939–45 war, Field Marshal Smuts, F.R.S., said in one of his speeches that " Mankind had folded its tents and was on the move ". *Turning Points in Physics*, which is the title of a series of lectures given at Oxford University in Trinity Term 1958, reflects a similar movement in physics which began at the turn of the nineteenth century and has gathered momentum during the years of the present century. It is necessary from time to time to re-think the basis upon which physics is built. Sir Oliver Lodge's definition of physics was ' organized common sense ' but the experimental discoveries concerning atomic structure of the present century, and the theoretical attempts to explain them, would not fit this definition in the accepted sense of the term.

The volume under review is a most welcome appraisal of this new attempt to unravel the secrets of nature and to point out the mistakes which have had to be corrected in the old ways of thought. There are six chapters which deal in turn with ' The End of Mechanistic Philosophy and the Rise of Field Theory ', ' The Quantum Nature of Radiation and Matter ', ' Probability ', ' Relativity ', ' The Decline and Fall of Causality ' and, finally, ' The Elementary Particles in Nuclear Physics '. Dr. A. C. Crombie introduces the subject with a reference to Tyndall's account of Faraday's distrust of symbols and analogies. Faraday consulted Whewell, the Master of Trinity College, Cambridge, in the matter of terminology for the new subject of electro-chemistry. Just as the investigations into the nature of motion and inertia by Newton meant the replacement of age-old concepts of Aristotle, so, in our present-day physics, ideas taken for granted have had to be jettisoned.

R. J. Blin-Stoyle begins with a clear explanation of physical theory. We observe, we make experiments and formulate a theory to explain the observed facts and predict new ones. The so-called ' Mechanistic Philosophy ' grew from the very success of the work of Copernicus, Galileo, Kepler and Newton. The latter formulated his Laws of Motion and then was able to postulate the gravitational force between bodies and how it varied with distance (Inverse Square Law) and so explain the motion of the planets, the tides, perturbations, and so forth. The solar system appeared as a gigantic machine moving elegantly and according to fixed laws.

Two exciting discoveries of Coulomb and Mitchell, showing that in electricity and magnetism an ' Inverse Square Law ' also held between charges and magnetic poles, seemed to strengthen the cause of Mechanistic Philosophy. In the phenomena of heat and light it took some time to show they were forms of energy though the work of Young and Fresnel had demonstrated the wave nature of light, and Joule's experimental work had ensured the demise of the caloric theory. The attempt to explain the wave theory of light by postulating the ' aether ' led to insuperable difficulties and provided the spur for Faraday and, later, Maxwell's Field Theories. Faraday's last researches into the connection between light and magnetism strengthened ' Field Physics ', and were followed by Maxwell's discovery of electromagnetic waves and Hertz's demonstrations of their existence and properties. But as Blin-Stoyle says of the Field Theory: " as it became clear that it was a description of the field rather than its sources that was essential for a complete understanding of the behaviour of the sources it was realized that a new concept was arising for which there was no place in the old Mechanistic Philosophy ".

In Chapter II, Dr. D. ter Haar deals with the rise of the Quantum Theory from Planck's success (1900) in explaining the energy/wavelength curves in ' Full Radiation '. All progress to that time rested on arriving at the particular ' model ' and the tacit assumption that what held for bodies in bulk with which Newton worked (that is the macroscopic world) would hold for the microscopic atom. Einstein's explanation of the photoelectric effect in 1905 was successfully based on Planck's idea of light energy quanta. Through the work of Rutherford, Bohr and de Broglie, the particle–wave dualism presented a problem which led to the successful wave mechanics theory of Schrödinger and the contributions of Heisenberg, Dirac and Kramer in solving many of the inconsistencies of the older theoretical physics. From this chapter we realize the immense stature of Bohr, and the magnitude of his influence and contributions to the subject. The quartet of names: Planck, Einstein, Rutherford and Bohr, stand out like giants, though the reviewer was surprised to learn that Rutherford (the most forceful of the four) so dominated the practical scene in Britain that " theoretical physics is not as well developed as experimental physics ". Bohr is " always questioning, never certain of the answers. . . . He is continually fighting to express himself, and his papers clearly break the rule that what is not clearly stated is not understood and should not be said ".

K. Mendelssohn's key note in Chapter III is an exposition of the true nature of physics —" a philosophical method . . . which relies on rigorous quantitative relations between observations ". Dangers to avoid are the pressing of analogies too far, i.e. introducing analogies with everyday experience which have no basis in observation. The advent of heat engines, the work of Joule, Maxwell, Boltzmann and Clausius, led to the formulation of the laws of thermodynamics and, in particular, the true nature of entropy. Statistical methods rely on large numbers and Maxwell successfully developed the Kinetic Theory on relatively few assumptions concerning molecules and the fact that heat is their mechanical energy. Then the assumption of " processes always going in the direction which led to the development of heat " was shown to be fallacious. It was the increase in entropy which was the governing factor and it was to Boltzmann's credit that he " explained it statistically as (the logarithm of) the probability of a state ". It measures the degree of disorder. Later, Nernst was able to follow this with a clear picture that at absolute zero there is zero point of disorder—a very different conception from the old idea of no motion. Dr. Mendelssohn was a student during the difficult ' Twenties '—a time of utter confusion in physics—and he attended lectures by Planck, so he writes of the relief brought about by de Broglie's triumph in demonstrating the wave nature of particles. As with many great advances, it was the genius who could pose the right question. The year 1927 was that in which " the contradictory aspect of the particle-wave dualism " was solved. The real difficulty arose when it was necessary to measure the momentum and position of an elementary particle such as an electron. Schrödinger had shown that the matter wave indicates the probability of finding the particle. So " wave mechanics and the uncertainty principle at once supplied the meaning of the quantum structure of the atom ". The somewhat naïve model atom with its mixture of classical and quantum properties, e.g. electron orbits without radiation, was abandoned. As for the uncertainty principle its meaning is realized in the fact that " the laws of quantum physics are of a statistical nature. We are reaching the limit of physical reality and must ever remain uncertain about individual physical events within the limits of the quantum constant ". It is of the order of 10^{-27} c.g.s. units so it is our very size which saves us from indeterminacy as our perception is statistical. Dr. Mendelssohn concludes: " The fundamental ideas of modern physics are based on the fact that truth is statistical ".

No treatment of modern mathematical physics is complete without Einstein's great contributions of the Special and later General Theories of Relativity which the reviewer remembers forming the subject of popular newspaper articles just after the 1914–18 war. Professor Temple, to use his own words, " speaks first of Newtonian relativity and of what I can only call Maxwellian absolutism. The clash between these two, or rather the resolution of the clash, I shall speak about in relation to Einstein's theory of relativity, passing on to deal with Minkowski's geometrization of the theory . . . and the complete relativity of Whitehead ". Professor Temple goes back to fundamentals in Newton's postulates and brings out the essence of the ' Third Law ' that " you can only make progress in the face of ignorance by assuming that all interactions (holding a body together) cancel out ". Accelerations and forces are measured with reference to an inertial frame, i.e. one where there are no unbalanced forces. The paradox of the Newtonian situation lies in the doctrine of absolute space and time side by side with an implicit principle of relativity.

Professor Temple next considers the elegant work of Maxwell whose equations in the electromagnetic theory led to the prediction that electromagnetic waves travel *in vacuo* with the velocity of light. (Velocity equal numerically to the ratio of the electromagnetic to the electrostatic unit of charge.) No mention of the frame of reference is needed; this fact, and others such as the null effect of the Michelson & Morley experiment, was met by Einstein in the Special theory. But more than this, Einstein went to the origin of a physicist's actions in carrying out measurements in space and time by asking " What are they doing ? " It is not investigating properties but carrying out operations. He had to analyse what is involved in measurements of space and time and answer such difficult questions as the identical nature of seconds, and what are rigid rods. A third contribution was his development of the invariance of the speed of light when measured by observers in relative motion.

We are next introduced to Minkowski's and Whitehead's contributions via the two geometries (*a*) mathematical geometry of Euclid and (*b*) physical geometry, where " the criterion is consistency with the brutal facts of this vale of tears ". Professor Temple's view is that " It is the experimental basis which ought to engage our attention—such topics as the optics of moving sources and media, and careful consideration of the modern repetition of the experiments of Michelson and Morley ".

Dr. F. Waismann, Reader in the Philosophy of Science at Oxford University, in the longest chapter (about a third of the book), deals with the demise of causality. To a certain degree he recapitulates some of the arguments in earlier chapters, distinguishing between ' cause-and-effect ' and functional dependence. The gas laws and Kepler's laws are two illustrations of the latter, and "such laws enable us to derive and thus predict the future from the present, generally the unknown from the known ". Galileo began the change in thought by asking *how* rather than *why* bodies fall. Determinism and the stability of the solar system culminated in the formulations of Laplace, and it was not finally disposed of until the establishment of the uncertainty principle in 1927. In a careful analysis of what is meant by law in science Dr. Waismann shows that " Far from aiming at producing a faithful and detailed copy of reality, science is an endeavour to comprehend it. And this it does, firstly by analysing it into component parts, then seeking for laws to connect them, and finally reconstructing reality out of these elements ". The very idea of law in science is of relatively recent origin, flowering in the period of the Rationalists of the eighteenth century, though the word has a long history as used, say, by the Greeks or the Hebrews. Again, inherent in the determinism of Laplace is the assumption that unlimited accuracy is within our reach.

When we descend to the atomic scale of events and times, we are in a way handicapped because terms such as particle, and the attributes position, velocity, momentum, energy, imply an accurate knowledge of details. Heisenberg showed " not only that there is no absolute precision but a definite *limit* to it, depending on the experimental situation, i.e. (*uncertainty as to position*) × (*uncertainty as to momentum*) = *a constant* (h) where $h = 6{\cdot}625 \times 10^{-27}$ erg . sec. We now see why classical physics was powerless to solve the problems of atomic processes. In the latter *it is impossible to predict* the result of an observation with certainty; what can be predicted is only the probability of any such result ". Photons represent only one aspect of light, and the author establishes in a simple way the Heisenberg relation that it is impossible to get hold of a photon which possesses (*a*) a sharply defined position and (*b*) a sharply defined momentum, and the same holds for material particles. Thus the modern view shows us that nature in intimate detail is unpredictable, yet on the average there are marked regularities. Finally Dr. Waismann considers the limitations of common logic, which is not exempt from the pressure of facts.

In the last chapter Professor Wilkinson gives a most interesting description of the elementary particles, i.e. the new fundamental particles and the part they play in nuclear physics. After a short exposition of the deficiencies of language in describing new concepts, particularly in dealing with phenomena in relationship to the nucleus in time and space, we are warned against the deceptiveness of pressing analogies too far.

First certain concepts such as energy, linear and angular momentum are conserved, a fact deducible from symmetry principles. β particle decay and the apparent violation of the conservation laws of energy and momentum led to the discovery of the neutrino. A striking fact, which is not yet understood, is the quantization of charge, for we cannot have an atomic theory of matter without an atomic theory of charge. From conservation principles we are introduced to the baryon, i.e. either a neutron, proton or hyperon. The latter is of mass greater than the neutron or proton and is highly unstable. To every baryon there is an anti-baryon. Dirac first postulated anti-particles, e.g. the positive electron as the anti-particle of the electron, and the anti-proton which has a negative charge but is otherwise identical with the proton. Conservation means the total baryons minus the sum of the anti-baryons in the universe is a constant. Next come the leptons which obey a similar conservation law. The leptons are made up of the positive and negative electron, the neutrino and anti-neutrino, and positive and negative μ-mesons; the latter have a mass 207 times greater than the electron. We are then left with other particles of masses greater than the electron decaying into simpler units, and there is also the photon; there is no indication as yet why the conservation law does not hold for these units, but the difference in the two types is connected with their intrinsic angular momentum. Thus for those particles which obey conservation laws—called the fermions, their angular momentum is $\frac{1}{2}$ in units of Planck's constant divided by 2π, while the others, the bosons, have integral spin 0 or 1 unit.

Finally we have a consideration of the terms Isotopic Spin, Parity and Strangeness, all of which assist in classifying the relation between the particles, their interactions and length of life. Naturally, the picture is not complete and we appear to be in the position of the chemists a century ago when the Periodic Table was first suggested.

The reviewer found this book stimulating, though the assertion that the lectures can be understood by laymen as they are non-technical is not fully justified. Only those with an understanding of classical physics and the more recent physics of the first three decades of the present century will get a clear picture. At some future date a companion volume showing how the practical results were obtained would form an agreeable complement. It would have fortified the last chapter to present an account of the modern devices such as the cloud chamber, diffusion chamber, bubble chamber, and the photographic film techniques upon which the theoretical deductions have been made.

These are minor criticisms, for those who read the book will be well rewarded. They will gain insight into the methods of approach to modern theoretical physics and, in particular, its application in unravelling part of the mystery of the atom. There is still much to be done and a future full of exciting possibilities. F. OLDHAM.

Reports on Progress in Physics, Volume XXII. (The Physical Society, London, 1959.) [Pp. iv + 624.] 63*s*.

The Physical Society has recently published its twenty-second annual volume of *Reports on Progress in Physics*. After having looked at it, I was moved to look at the first volume of the series, for comparison. Volume I consisted of 370 pages, and contained a dozen articles—some sub-divided into self-standing sections—including among them titles such as ' Optics '. ' Spectroscopy ', ' X-rays ' and ' The Physics of the Atom '. Most of it, but perhaps not all of it, would have been comprehensible to a physicist who had taken a first degree with honours in the not-too-distant past. Volume XXII consists of 630 pages and contains 14 articles; choosing titles to emphasize the contrast I wish to make, we have ' Double Beta Decay ', ' The Theory of a Fermi Liquid ' and ' The Propagation of Elastic Waves in Crystals and other Anisotropic Media '. With one possible exception, I doubt if any of the articles could be read by a recent graduate without a great deal of intellectual effort, while some of them will be incomprehensible to all but specialists in the relevant field of research.

The change is to some extent inevitable. One cause is the rapidly increasing content of that branch of learning included under the heading of ' Physics '. The last physicist must have died towards the end of the nineteenth century: perhaps his name was Lord Rayleigh. Since then we have, individually, been learning more and more about less and less, and the tendency can be traced by looking at the contents lists of successive issues of ' Progress Reports '.

It is this same rapid growth which has called forth the stream of review articles and progress reports from various sources, and which is determining their character. A continuing series of annual reports on progress in ' Heat ' would, if their title were at all apt, rapidly become no more than an annotated card-index, lacking unity and coherence: the change to more specialized subjects is to be expected. On these narrower fronts, one can usually distinguish between a review article and a progress report. The former, I would suggest, should be written primarily with the non-specialist reader in mind; should be capable of being read, rather than studied; should present the subject in perspective; and should not exclude broad generalizations and the expression of opinions which may well be subsequently proved wrong. A progress report will appeal not only to the specialist in the field covered, but also to specialists in related fields who will at least speak the same language; may quite well take up the subject at the point where the last published report on a similar field left it; will form a useful starting-point for a student commencing work in the field; and should not be averse to pointing out contradictions without attempting to resolve them. There is a need for both types, and almost all of the articles in the present volume approximate more closely to the second type. One consequence is that every article will appeal to its own particular circle of readers, and the fourteen circles may not overlap very much. It is doubtless with this in mind that The Physical Society has in the past few years decided to issue individual articles separately at prices depending on their length.

Of the individual articles in the present volume there is one which differs from the remainder by having more of the character of a review. ‘ Relativity ’ by H. Bondi (22 pages) starts from a point which should present no difficulty to any physicist, and concludes with a section on modern developments. The treatment is, as far as is humanly possible, non-mathematical. The very nature of the subject-matter ensures that it is not light reading: but the average reader should be able to understand most of it, and at least catch a glimpse of the meaning of the remainder. At the other extreme, ‘ Paramagnetic Resonance Data ’ by J. W. Orton (35 pages) consists, except for two pages of explanation, of a table of experimental data, which will have meaning and utility only to research workers in this specialized field. For such people the table is doubtless very valuable, containing as it does some unpublished results and some others not readily accessible; it is, in fact, a kind of appendix to two previous articles on the same subject which appeared in *Reports on Progress* for 1953 and 1955 respectively.

Of the remainder, which lie somewhere between the two extremes, those on experimental subjects were the more comprehensible to the present writer—a fact possibly not unconnected with his inadequate mathematical competence. ‘ Experimental work with ^{3}He ’ translated from the Russian of V. P. Peshkov and K. N. Zinov’eva (54 pages) is a comprehensive summary of a very large amount of work dealing mainly with the physical properties of ^{3}He, but covering also its production and possible uses in low-temperature research. Many of the results are given in detail, either as graphs or tables of data, and the article is one of the few which include diagrams of apparatus. Anybody in search of information on the subject covered by the title will find it here, set out clearly and in detail. ‘ Progress in Vacuum Technology ’ by J. Pollard (38 pages) is also comprehensive, covering not only pumps and pressure gauges but other topics such as leak detection and very low pressure techniques. The author’s declared intention is to cover advances made in the last two decades and this he does very thoroughly. The general reader should be able to cope with most of it although just occasionally a more intimate previous knowledge is called for (should I have known what a Penning gauge is ?); the specialist will find it a useful survey.

‘ Experimental Investigations of the Ionospheric E-layer ’ by B. J. Robinson (35 pages) somewhat belies its title. It says almost nothing about experimental techniques, and is concerned largely with summarizing experimental results, and with interpreting them in terms of existing theories which are not explained. The reader unfamiliar with the techniques and the theories will probably find it necessary to read up the subject in earlier publications (to which some references are given) before beginning on this survey.

‘ The Propagation of Elastic Waves in Crystals and other Anisotropic Media ’ by M. J. P. Musgrave (20 pages) is mathematical in its approach—as is inevitable—and is not easy reading. Some comparison of the mathematical results with experimental observation is made, but one of the most striking facts that emerges is that this essentially classical—though difficult—piece of mathematics has not previously been done, and that this is partly because, until recently, no experimental techniques were available for making the corresponding observations.

‘ Physical Problems of Thermoelectricity ’ by A. F. Joffe and L. S. Stil’bans (from the Russian—35 pages) is concerned with materials suitable for use in thermoelectric generators of electricity. A very brief introduction establishes the desirable features—high thermo-e.m.f. and electrical conductivity, low thermal conductivity—and the remainder of the paper discusses these quantities in terms of the concepts of solid-state physics, with particular reference to semiconductors, and gives the results of measurements designed to confirm the theories. Readers with a previous familiarity with semiconductor theory will have an advantage over others.

' Nuclear Magnetism in Pure Liquids ' by J. G. Powles (66 pages) begins by referring the reader to the " many reviews and books " on nuclear magnetic resonance and, presuming that these have been read, embarks on a discussion of the problems peculiar to the particular aspects covered by the title, after a brief introduction which is, presumably, not necessary for the expert and, more certainly, little assistance to the non-expert. There are many references to experimental results which are discussed in connection with the theory.

' The Theory of a Fermi Liquid ' by A. A. Abrikosov and I. M. Khalatnikov (36 pages) is the third Russian contribution, and bears the sub-title ' The Properties of Liquid ^{3}He at Low Temperatures '. It is a companion paper to the one mentioned earlier dealing with experimental results and is entirely theoretical and mathematical.

' Applications of Quantum Mechanics in Theoretical Chemistry ' by G. G. Hall (28 pages) is also theoretical, but avoids an excess of detailed mathematics and does attempt to explain the nature of the problems and the broad lines upon which they are attacked, in English. Even those who cannot tell a spin operator from an eigenfunction should be able to profit by reading this article.

' Radio-frequency Spectroscopy of Excited Atoms ' by G. W. Series (46 pages) is again very specialized. After a highly condensed summary of experimental techniques and a rather longer summary of some relevant parts of spectroscopic theory, there follows a section on ' double resonance experiments ' which are discussed in detail.

There are finally three contributions on elementary particles. ' The Two-nucleon Interaction ' by R. J. N. Phillips (65 pages) is entirely theoretical and is a survey of recent progress towards an understanding of the forces between two nucleons. " The chapter on phenomenology is meant to be intelligible to any reader with a working knowledge of quantum mechanics, whereas the chapter on meson field theory pre-supposes a familiarity with modern field theory." *Verb sap.*

' Double Beta Decay ' by H. Primakoff and S. P. Rosen (43 pages) is remarkable for the fact that it is about a phenomenon that has never been observed. The introduction explains why the phenomenon is important in nuclear physics, and attempts to observe it are described in some detail; rather more than half the space is devoted to the relevant theory—an exercise which produces an equation extending over more than a page, even in the condensed notation used.

' Invariance in Elementary Particle Physics ' by N. Kemmer, J. C. Polkinghorne and D. L. Pursey (62 pages) deals with one of the most intriguing developments of modern theory. One facet of this is the question—as the authors express it ' loosely '—of the equivalence of left-handed and right-handed descriptions of physical processes; the related concepts of ' charge conjugation ' and ' time reversal ' follow close behind and we rapidly move into problems of ' isobaric spin ' and ' strangeness number '. The two-page introduction whets the appetite; but unless the reader is at home with unitary operators, Hermitian conjugates, commutators, state vectors and Hamiltonians, he will not get more than half a page further.

It may well be that it is impossible to write an article of a permitted length on a topic as specialized as those included in this volume, which starts from a point where the non-specialist can understand and finishes beyond the point where the specialist has become interested. If the character of the volume dictates that the latter condition must be satisfied, it would be a gracious gesture if a little space could be spared to tell the rest of us what it is all about and, more particularly, where we can find the beginners' book, the intermediate course and the moderately advanced account. Some authors do this; others—among them the writers of the two last-named articles—make no concession.

N. Thompson.

Thermonuclear Processes. A Convention held jointly by the Institution of Electrical Engineers and the British Nuclear Energy Conference in April 1959.

The convention, the first in this country on controlled fusion research, dealt largely with the scientific and engineering work carried out in Britain, with contributions from the U.S.A. and U.S.S.R. The object of the work is to heat electrically a deuterium or deuterium–tritium gas mixture to a sufficiently high temperature so that the resultant fusion energy released by the D–D or D–T reactions is at least equal to the electrical input. No one has yet reached this ' break-even ' temperature, which is in the region of 10^8 °K.

The convention was opened by Sir George Thomson, after which T. E. Allibone and D. R. Chick presented a paper on the basic physics of fusion processes and plasma containment. They concluded that our theoretical knowledge of plasma behaviour is insufficient, and that some experimental results are contradictory to present ideas of plasma physics. D. W. Fry reviewed the whole field of experimental work conducted up to the present time. He described the pinch machines, upon which Britain has concentrated her efforts. The axial current type, typified by Zeta and Sceptre, consist of a torus filled with a deuterium plasma acting as a single-turn secondary of a transformer. An axial current is induced by discharging a condenser bank into the primary, and the azimuthal magnetic field due to the current causes the current-carrying plasma to contract or pinch towards the axis. Thus the self-magnetic field of the plasma current acts as a form of magnetic bottle for the hot plasma by keeping it away from the walls.

Heating is due to normal resistive heating of the plasma by its induced current. The azimuthal current type, typified by the A.W.R.E. machine, has azimuthal plasma currents induced which produce an axial magnetic field. The effect is again to cause the plasma to contract towards the centre and provide magnetic containment of the hot gas. The axial current type of pinched discharge is subject to instabilities which limit the containment time, and although the most violent forms of these can be suppressed by an axial stabilizing field the instability problem has not been fully overcome.

Dr. Chester van Atta reviewed the American work. A large part of their effort has been concentrated on the Stellarator type of machine in which plasma containment in the torus is effected by an axial magnetic field supplied externally. This does not rely upon the self-magnetic field of the plasma current. Stability problems are also present in these machines due to charge separation, giving rise to a transverse electric field which prevents plasma equilibrium.

The magnetic mirror machines being investigated in the U.S.A. and U.S.S.R. were described in convention lectures. These machines depend upon the use, in an open-ended system, of a magnetic field whose intensity rises at each end to a value above that at the centre. A beam of ions is injected into the field, and the field gradient has the effect of reflecting the ions at the ends and so trapping them. As yet no stability problems have arisen, but difficult technical problems need solution before further progress can be made.

A number of papers were presented dealing with engineering problems of transformers and switches. The necessity for switching megamperes in microseconds indicates that the nuclear and plasma physicists do not have a monopoly of problems, and that the engineering of thermonuclear machines will call for great ingenuity.

G. B. F. Niblett described the principle of using azimuthal currents with axial magnetic fields, employing a rapidly rising field to produce shock heating of the plasma. The azimuthal current sheet begins near the inside surface of the torus and is rapidly driven inwards by its magnetic field, with the production of shock heating. He stressed the importance of producing a conducting gas before application of the discharge, and indicated that the instabilities of such a system are less difficult than those of the pinched axial discharge.

The direct conversion of fusion energy to electrical energy was discussed by J. D. Jukes. Direct conversion is possible because of the presence of currents and magnetic fields which are linked by transformer action to an external circuit. He gave a circuit diagram and switching sequence which illustrates the modulation of the discharge current about a mean value I_0. During part of the cycle work is done on the plasma by the confining magnetic field, compressing the plasma and increasing temperature and hence fusion rate. During subsequent adiabatic expansion work is done by the plasma in the magnetic field, this work appearing as available energy in the external circuit. He discussed methods of drawing off the energy, and concluded that it is possible to convert one-third of the total energy in this way.

The overall impression of the convention is that the emphasis of future research has moved towards understanding the basic physics of plasma processes, and that a significant step forward will not be made until these processes are better known.

The papers, lectures, and discussions are published as Supplement (No. 2) to Vol. 106, Part A, of the *Proceedings of the Institution of Electrical Engineers.*

J. Thomson.

BOOK REVIEWS

Reading German for Scientists. By H. Eichner and H. Hein. (London: Chapman and Hall Ltd., 1959.) [Pp. xi+207.] 30*s.*

This is an excellent book. The authors have set themselves what is from the linguistic point of view a limited target and they attain it most successfully. At first sight to a teacher of modern languages a book which almost entirely ignores the spoken element seems ‘ wrong ’, but in its context the work has such authority and intelligence that it commands respect, and incidentally offers a number of useful hints to teachers of the spoken tongue.

In the first place the book has the ring of experience; it has been tried in practice (in the form of mimeographed sheets at the Royal Military College of Canada and in University work) and proved successful. Secondly, the treatment of grammatical problems is done with commendable lucidity; particularly admirable are the sections on the ‘ extended modifier ’ construction and the anticipative use of prepositional compounds, which are clearly propounded with a wealth of example. Thirdly, the examples themselves, and the sentence tests, prove paradoxically to possess just that realism which is so often missing in conventional language courses, because of their close relation to practical experience. “ The water is boiling in the test-tube ” (lesson III) seems vastly more interesting, in its context, than a good deal of the standard ‘ pen of the gardener's aunt ’ material. The chapter on Aids to Vocabulary Building is an extremely sensible and practical series of hints on accurate guessing of German meanings, and invaluable to the science student.

By intelligent use of boxing and heavy type the authors present each chapter in the most telling and digestible manner. The reading matter is all drawn from established scientific works and the illustrations appear efficient. The last third of the book consists entirely of readings in Chemistry and Physics and here, as throughout, the provision of vocabulary notes is thorough and easily accessible.

In short the fascinating world of Phosphorwasserstoffgeruch and Benzolsulfochlorid is opened up with exemplary thoroughness and commonsense—albeit at a price which might sometimes prove prohibitive—and *Reading German for Scientists* caters admirably for the student who wants to be able to read German scientific texts and wishes to reach this limited aim by the shortest route.

D. A. Emms.

The Many-body Problem. A symposium. (London: Methuen Ltd., 1959.) 100*s.*

This volume contains the lectures given at last year's Summer School at Les Houches which was devoted to the many-body problem. In accord with the very advanced level of this Summer School, the contents of this volume are definitely not for the general layman. Indeed, I should expect that only those theoretical physicists who are actively working in the field can use these notes with profit, but for them they constitute a veritable gold-mine of information. The authors represent nearly all major centres of research on the topic: Hugenholtz (Utrecht), Brueckner (Philadelphia), Thouless (Cornell), Bloch and De Dominicis (Saclay), Mottelson (Copenhagen), Weisskopf (M.I.T.), Stroutinsky (Moscow), Lipkin (Rehovoth), Beliaev (Moscow), Bohm (Bristol), Pines (Princeton), Schrieffer (Birmingham), Huang (M.I.T.), while Lynton (Columbus, Ohio) gave a survey of experimental data on superconductivity and liquid helium. The range of topics covered is also most impressive, varying from the electron gas, plasmas, liquid helium, ^{3}He, and superconductivity to nuclear theory; indeed, something for almost everybody. My own regret is that there is not a set of lectures on the many-body problem given by a less high-powered theorist which could have set the stage for the many theoreticians who wish to get acquainted with the field and which at the same time could have given a comparison between the different methods of approach (neither Tomonaga's method nor Yevick and Percus' work is, for instance, discussed anywhere).

A word of warning to anybody intending to read this book: in order to benefit from it one must be well versed in the techniques of modern quantum mechanics and field theory.

Finally, although there is an extensive table of contents there is no index and the price seems rather steep for a book produced by photocopying mimeographed lecture notes—even if it is in luxury binding.

D. ter Haar.

Čerenkov Radiation and its Applications. By J. V. Jelley. (Pergamon Press, 1958.) [Pp. x+304.] 65*s*.

This book, as yet the only monograph dealing exclusively with Čerenkov radiation, is written primarily for the experimentalist specializing in this field. Its author, a pioneer in the development and practical application of the Čerenkov counter as a precision instrument, has produced a stimulating account of a phenomenon which, although of limited academic interest, has made important contributions to a wide range of physics. The scope of the volume under review may be judged from its chapter headings: Introduction; Theoretical interpretation; Extensions to the theory; Later experimental work; The photomultiplier; Čerenkov detectors and their applications; Optical considerations; Designs of some practical counters; Čerenkov radiation in the atmosphere; Gas counters; Miscellaneous ideas and applications. The text is supplemented by useful appendices containing numerical and graphical data, and by an excellent bibliography.

The monograph will constitute an invaluable work of reference for scientists specifically interested in the problem of detecting fast charged particles. Non-specialists will readily be able to abstract from it a general idea, at the depth of an elementary review article, of the present state of the subject. Any competent physicist could, for example, appreciate the description given of experimental investigations confirming the Čerenkov effect, the discussion of the merits and limitations of the Čerenkov detector as compared with the scintillation counter, the review of the development of low-intensity standard light sources based on the phenomenon, and the appraisal of the possible rôles of Čerenkov radiation in contributing to the light of the night sky and to the emission of radio waves from sunspots.

As far as experimental and technical matters are concerned, Dr. Jelley's monograph is admirable. Unfortunately, the same can hardly be said of its theoretical side. The author has, quite properly, omitted the elaborate mathematical analysis of the classical Čerenkov effect in anisotropic and in optically active media: and, very wisely, he has not attempted, at other than superficial level, a discussion of the quantum-mechanical interpretation of the Čerenkov phenomenon. The Frank–Tamm classical theory of the radiation emitted by an electron moving rapidly through an isotropic dielectric could, however, have been presented in a far more *anschaulich* manner than is here done. Some, but by no means all, of the obscurity arises from the fact that the author never specifies his units; and, while some relations are, in fact, expressed in terms of the rationalized m.k.s. system, others employ the unrationalized mixed (or Gaussian) system. Thanks to this latent confusion, care is necessary when interpreting formulae. To quote one example: in equation (10.1) the units are rationalized, and the symbol ϵ_0 (which, incidentally, should appear in the denominator rather than the numerator) denotes the m.k.s. value of the dielectric constant of free space, and not the static dielectric constant of the medium, as implied in the text. It is to be hoped that, when revising the volume for subsequent editions, Dr. Jelley will bring his treatment of Čerenkov radiation theory up to the high standard of the remainder of his monograph.

C. Hurst.

Basic Practical Physics. By E. Armitage. (London: Cambridge University Press, 1959.) [Pp. 80.] 5*s*.

The book gives detailed guidance for the performance of 26 basic experiments covering the ' O ' level course for the General Certificate of Education. Each of these is followed by a few questions from examination papers in practical physics which represent variations on the basic themes and give a pupil opportunity to test his grasp of principles and acquired skill in slightly unfamiliar situations. There are also 23 miscellaneous practical examination questions. General notes on practical work, hints on the writing up of experiments, and tables of logarithms, reciprocals and natural sines are included.

The author emphasizes his belief that practical work is the basis on which the foundation of physics should be built, and implies that experimental work tends to be neglected in schools which do not have to prepare pupils for a special practical examination. This criticism, if justified, is certainly a very serious one. But will a book which itself emphasizes so strongly the examination aspect of practical work serve to achieve the close correlation of classroom and laboratory studies through which practical physics can perform its true function in the scientific education of the pupil?

W. Llowarch.

Basic Electricity. By Van Valkenburgh, Nooger, and Neville. (London: Technical Press, Ltd., 1958.) In five parts: part 1, 127 pp.; part 2, 122 pp.; part 3, 122 pp.; part 4, 104 pp.; part 5, 117 pp., with index. 12*s.* 6*d.* per part.

One of the facts of modern times is that scientific apparatus not only enters into the lives of non-scientists, but often has to be operated and serviced by them. Their training in such scientific principles as are considered necessary demands a special approach. This book offers one.

It is illustrated with cartoons which occupy much more space than the letterpress. An example (designed to encourage the use of electrical apparatus at its rated voltage) shows a motor with a relaxed face on its end-plate, saying, " Ah ! 110 volts ! " ; another motor agonized and perspiring, gasping, " Gadzooks ! 220 volts ! " ; and a third with a frustrated expression, and in its balloon, " 50 volts. . . . Gosh ! can't move ! "

The preface claims ' brilliant simplification '. As often happens, this involves a certain loss of accuracy, which, in this context, may be of no importance. For instance, an air-coil core is shown as having 10 lines of magnetic flux linking it. When, with the same current in the coil, an iron core is inserted, the lines inside the coil are concentrated into what looks like a single fat line, but there are still only 10 lines in the return path.

The approach is naïve, and is clearly meant to be so. Whether it succeeds in its aim can only be shown by tests on the right material. It was developed for the U.S. Navy and published in 1954. The preface claims a cut in the training of Naval Technicians by one-half.

The present edition has been modified for British use by a R.E.M.E. team.

G. F. Nicholson.

Experimental Techniques in Low Temperature Physics. By G. K. White. (London. Clarendon Press, Oxford University Press, 1959.) [Pp. vii + 328.] 45*s.*

During the last decade there has been a great increase in the amount of physical research being carried out at low temperatures ($<20^\circ$K). A number of things have combined to give this stimulus: the successful commercial manufacture of a helium liquefier in the U.S.A., the increased interest in solid state physics and more recently the interest of nuclear physicists in hydrogen and helium bubble chambers. The number of laboratories conducting experiments involving the use of liquid helium and hydrogen has increased ten-fold. In spite of this, much of the detailed ' know-how ' of low-temperature techniques has not been written in easily accessible places until the appearance of this book. It fills an ' aching void ' in the technical literature successfully.

The author has divided his book into three parts. The first deals with the liquefaction of hydrogen and helium, their storage and handling techniques, the basic mathematics and the construction of heat exchangers used in liquefiers, and the measurements of temperatures. There is a great deal of useful information which will be invaluable to the low-temperature tyro when first learning the specialized techniques of handling these most volatile of all liquids from which air must be excluded. There is also enough information to tempt the more adventurous spirits to design their own liquefiers if necessary.

In the second section, called ' The Research Cryostat ', the author deals with the problems involved in designing and building apparatus for experiments in the temperature range immediately above 1° absolute. He also gives a good introduction to adiabatic demagnetization techniques used to generate temperatures well below 1° absolute. In the last section he discusses and tabulates the physical data which are needed continually when designing cryostats and experiments, viz. heat capacities, expansion coefficients, electrical and thermal resistivities.

This book is intended to be and is an excellent primer for those who are entering or interested in the low-temperature field. The author has attempted to relate the techniques and data to the corresponding fundamental physical principles so giving an insight into what might otherwise appear as ' rules of thumb '. His points are illustrated with calculations and extensive references for further reading. The book was written in Canada, which is probably the reason why the author sometimes refers to American sources of materials and equipment without giving the British sources. He does not deal with the use of liquid ^{3}He to generate temperatures below 1° absolute. This is still a specialized technique using a somewhat rare isotope but seems an important omission. Nevertheless, this book, written by a very experienced low-temperature physicist, is good value indeed.

D. H. Parkinson.

Radio Studies of the Universe. By R. D. Davies and H. P. Palmer. (London: Routledge and Kegan Paul Ltd., 1959.) [Pp. xii + 200.] 25*s*.

The wide spectrum of radio wavelengths with which radio-astronomy adds to our knowledge of the heavens is now matched by a growing breadth of interest in this new subject. A superficial interest is easily satisfied by hearing occasional broadcasts, or by following that part of the daily press which treats radio-astronomy as worthy of some attention. But a coherent and full account we cannot easily find, even at the most technical level. As with any new subject, we must rely on the active participants to explain their work, and indeed they have a duty to do so. We should not expect great literary merit, but merely a straightforward account of work in progress. This is the task undertaken by Davies and Palmer.

Their account is directed at scientifically-minded people, with some training in physics. The uninitiated would probably experience trouble with the flow of facts and figures, especially numbers which appear glibly as powers of ten or when metric units and parsecs dominate the scene with only occasional references back to ounces, miles, and light-years. But the facts are certainly there, and we can be sure they are right.

Achieving a balance in such a wide canvas is difficult, and we must not complain about a local flavour or colour to the book. Some more of the most exciting work on radio stars, which has largely been carried on at observatories other than Jodrell Bank, might for example have been included. But a complete colour-blindness could be the only cause of the implication on p. 31 that the term ‘ radio-telescope ’ refers only to that particular form of construction in which radio waves are focused by a large reflecting surface on to a single dipole. Indeed the largest radio telescopes are not made in this way.

Meteors and comets provide surer ground, for Jodrell Bank was founded on the study of meteor echoes, and its long tradition is reflected in some excellent chapters.

F. G. Smith.

Space research

by H. S. W. MASSEY
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1. THE NATURE OF SPACE RESEARCH

As considerable confusion exists about the meaning of ' space research ' it is best to begin by explaining at least what it means to the writer. The study of the earth's atmosphere at first could only be carried out using ground-based instruments. Somewhat later it became possible by using balloons to carry observing equipment to altitudes well above those of the highest mountains. However, the maximum altitude attainable in this way is still not much above 100 000 feet (30 km). There is a great deal of atmosphere with interesting and important properties above this height. This became accessible to direct observation thanks to the remarkable development of high power rocket motors which was carried out, particularly in Germany, during the last war. Thus the V2 rocket was capable, when fired vertically, of carrying a load of several hundred pounds to a height of 150 km. No limitation of principle prevented the attainment of higher altitudes. It can fairly be said that with the first utilization of V2 rockets to carry scientific instruments into the high atmosphere, space research was born.

There is no reason why we should think of this research as concerned only with the study of the earth's high atmosphere. The strong influence of the atmosphere on incoming radiations is well known. For example, radiation in the far ultra-violet cannot penetrate much below the 100 km level and a similar restriction, partly due to the earth's ionosphere and partly due to atmospheric absorption, is imposed on radio waves except in the 1 cm to 10 m wavelength band. We have therefore the opportunity with rocket transported instruments to observe the universe in all radiations. In this sense we are justified in using the term ' space ' to describe the type of research involved.

Before proceeding to a more detailed account of the scope of space research there is one further very important point to make. In parallel with the extraordinarily rapid development of rocket engineering there has been an equally remarkable growth of techniques of electronic control and communication. Because of this it is practicable to carry out observations with instruments in space and to transmit the observations back to earth automatically. There is no need for the presence of human beings to carry out the experiment *in situ.* In short, space research in the sense in which we shall be using it throughout this article, is not involved with space travel. It is true, of course, that the scientific study of space as an environment is an essential prerequisite to manned travel in space but space research is a branch of science whose pursuit requires no more nor no less justification than any other branch of pure science. While it is not reasonable to justify manned space travel because of the scope it offers for carrying out scientific research, it is certainly incorrect to suppose that the aim of space research is merely to provide data for the design of manned space vehicles. It is one of the aims of this article to show what a wide range of important scientific problems can be tackled, and are now being tackled, through the technique of space research.

To sum up, we shall consider space research to mean the scientific study of the earth and the universe generally which may be carried out using instruments conveyed by the use of rocket motors to regions inaccessible in any other way. The unity of the subject is one of technique as it is concerned with many scientific disciplines such as geophysics, meteorology, geodesy, astrophysics, etc.

2. THE TECHNICAL PROBLEMS INVOLVED IN SPACE RESEARCH

The main problems involved in providing the necessary technical facilities for effective space research can be summarized as follows:

(1) The supply of rocket motors of sufficient power to transport a useful load of instruments to a sufficiently great distance from the earth.

(2) The provision of guidance mechanism which ensures that the instrument container follows a chosen path with tolerable accuracy.

(3) The need to have precise methods for following the actual path of an instrument container so that its position at any time can be determined with the necessary accuracy.

(4) The development of instruments which will operate satisfactorily under the unusual environmental conditions involved. This includes automatic operation without maintenance over periods of time of the order of a year.

(5) The provision of adequate power supplies within the instrument container.

(6) The inclusion of devices which can transmit the result of observations back to the earth.

We now consider these separately.

The requirements of rocket propulsion are best expressed in terms of the speed which must be attained by the instrument package. Thus to place an object in an orbit around the earth so that it circulates as an artificial satellite it is necessary to communicate to it a speed of 17 200 miles/hr. If this speed is increased $\sqrt{2}$ times to 24 300 miles/hr, the object can, if suitably guided, be placed in an orbit along which it passes out of the control of the earth's gravitation altogether. It will then become a new planet revolving in an orbit around the sun. To escape from the solar system altogether it would be necessary to communicate a much greater speed, near 100 000 miles/hr.

To provide these high speeds it is necessary to use multi-stage rockets. These consist of three or more rocket motors which fire in series. The second-stage motor does not ignite until the first has burnt out and dropped off, thus reducing very substantially the load which has to be further accelerated. Similarly the third stage does not ignite until the second stage has been completed and so on. Existing rocket motors used in this way have proved capable of communicating speeds sufficiently great to enrich the solar system with a new planet of mass several hundred pounds and of placing loads of a ton in orbit as artificial earth satellites. Even more powerful motors are in an advanced state of development, capable of landing a ton of equipment on the moon or placing tens of tons in orbit circling the earth at a distance of 300 miles or so. Within a few years these performances will certainly be exceeded.

It seems clear that the problem of providing rocket engines with sufficient thrust to take useful instrument packages anywhere within the solar system is virtually solved. The weight which may be transported is limited only by the financial and manpower resources which are available for the purpose of designing, developing, testing and producing the powerful rocket engines required.

There is little value in being able to provide the necessary thrust unless it is at the same time possible to control the operation so the object follows sufficiently closely a desired course. To place an object in an orbit as an artificial satellite it is essential that it be propelled, not only with a certain minimum speed, but also in a direction within a degree or so of the horizontal. This is a weak requirement compared to that which has to be imposed if the object is to make a rendezvous with the moon, while the problem is much more severe still if it has to pass close to Venus or Mars, to say nothing of more distant planets. Thus in order that an object should pass within 50 000 miles of the moon it must be projected with a speed within 150 miles/hr of a certain value which will be close to 22 000 miles/hr, in a direction within $\frac{1}{2}°$ of a prescribed one. It is evident that guidance accuracy of this order is already attainable from the fact that on 22 January 1959 an instrument container was launched from the Soviet Union into an orbit which passed within 10 000 miles of the moon. Difficult problems remain to be solved before it is possible to probe in the neighbourhood of any of the planets.

There are three methods available in general for tracking the paths of projected objects. Optical methods depend usually on observation, by suitable optical systems, of sunlight reflected from the object concerned. In some circumstances artificial sources of light may be incorporated, but up to the present these have not been used to any extent. For observations of artificial satellites optical methods are inherently capable of much greater precision than any other, but space probes, which penetrate out to the moon's orbit or beyond, become too faint to follow in this way. To extend the range of observation it is necessary to use radio direction finding, a radio beacon being incorporated with the instrument package. With the use of large receiving aerials, such as the radio-telescope at Jodrell Bank, it is possible to follow an object containing a radio beacon with a power of a few W over a bandwidth of a few c/s about a frequency of a few hundred Mc/s, out to the moon's orbit, as demonstrated during the flights of the American lunar probes. Radio communication was actually maintained with the American deep space probe Pioneer IV, which achieved escape velocity and became a solar satellite, out to a distance of 407 000 miles and this with equipment in the probe weighing only 13·4 lb. The limit is by no means reached in this direction but to extend the range from the present half a million miles to more than 50 times that distance is another matter. Apart from its important function in extending the range of observation, radio tracking also possesses the advantage of being independent of the position with respect to the sun. It also offers relative ease of acquisition. By this is meant the initial observation of the object after projection. It is much easier to scan a wide area of sky with radio-receiving apparatus than with optical equipment. On the other hand the radio location is less precise and there is the dependence on inboard power supplies.

Radar detection is also possible, provided the range is not too great, and has been employed with success with artificial satellites. If the radio beacon fails it may indeed be the only way of finding the object.

Linked with the problem of radio direction finding is that of data recovery. Although it is possible to recover records taken by instruments flown in nearly vertical rocket flights, this is somewhat unreliable. With artificial satellites which are destroyed on their return through the atmosphere, or with space probes which never return at all, one must use other methods of data recovery in any case. This is done by radio transmission of the observed results, in the form of a suitable code, back to ground receiving stations. This technique, known as telemetry, is highly developed and is usually employed in observations with vertical sounding rockets as well as with artificial satellites and space probes. It makes a considerable demand on battery power in addition to that required by the radio beacon. The distance from which successful telemetry can be carried out decreases with the complexity of the data to be communicated but it has already been done in practice out to distances of the order of 100 000 miles for admittedly rather simple observations.

The invention of the solar cell has greatly eased the problem of power supply in artificial satellites and space probes. No further proof of the effectiveness of these cells need be given when it is recalled that the so-called ‘ Grape-fruit ’ test sphere, 6 in. in diameter, launched in the U.S.A. in March 1958 into an orbit whose maximum (apogee) and minimum (perigee) distances from the earth were 2130 and 350 km respectively, contained solar cells which are still operating well enough to provide strong signals from the 108 Mc/s radio beacon. In fact, provision will have to be made in the near future, when satellites become numerous, for turning off their radio beacons on receipt of a command signal from ground or after a set time. Otherwise they will become a serious source of radio interference.

One essential requirement for successful space research is, of course, that the observing instruments should function correctly in the unusual environmental conditions which would be encountered. These conditions could be so severe as to make it difficult to meet this requirement. Thus the temperature within an artificial satellite if uncontrolled would vary over such wide limits that electrical equipment could not be used effectively. Apart from any heat generated by internal equipment the satellite temperature is determined by a balance between radiation emitted and absorbed by its surface. It becomes possible then to modify the properties of the surface so as to bring the internal temperature range within tolerable limits. This art of successful temperature control is a most important one. In a journey towards the sun, as for example in an attempt to probe the region near the planet Venus, allowance must be made for the fact that the heat received per square centimetre of surface from the sun substantially increases, and so on.

Another difficulty which must be reckoned with is the modification of the space environment due to the presence of the probing vehicle itself. This is particularly acute in making measurements of weak magnetic fields as only a minute amount of ferromagnetic material in the vehicle may completely vitiate the observations. It must be allowed for in observation of particle radiation, as secondaries will certainly be produced in the solid material of the probe. Early observations of atmospheric ionization using instruments in vertical sounding rockets were illusory because of electrical charges picked up by the rocket case or residual gas carried aloft, and so on.

In general the way in which a particular quantity is most conveniently measured by instruments contained within rocket-propelled vehicles or satellites is different from that which would be appropriate under ground laboratory conditions. Attention must be paid to ruggedness against acceleration and vibration, compactness and lightness, reliability and ease of adaptation to the use of telemetry, among many other considerations. Remarkable progress has been made in the design and construction of miniature and sub-miniature equipment. The introduction of the transistor has naturally been of great importance in this connection.

3. Scientific investigations which may be carried out

The scientific studies which may be carried out using rocket-propelled vehicles, including artificial satellites and space probes, may be considered under the following headings :

(1) Study of the high atmosphere of the earth.
(2) Meteorological studies.
(3) Study of the gravitational and magnetic fields of the earth.
(4) Astronomical observations from outside the atmosphere.
(5) Study of particle radiations in interplanetary space.
(6) Study of the composition and nature of the matter in interplanetary space including micrometeoric dust, neutral and ionized atoms and electrons.
(7) Studies of the moon and planets *in situ*.
(8) Investigation of the nature of gravitation.

The first space research was confined to the use of vertical sounding rockets such as the V2, the American Aerobee and Aerobee Hi, the Russian Meteorological and Geophysical Rockets, the British Skylark, to name but a few. This technique suffers from the disadvantage that the flight time is a few minutes at most, so that it can give only a snapshot of the vertical distribution of any quantity of importance. Nevertheless it has provided very many important results, some of which will be mentioned below, particularly in regard to items (1) and (4). It would seem at first sight that the availability of artificial satellites as vehicles must render vertical rocket sounding obsolete but this is by no means the case. A satellite placed in an orbit which approaches the earth closer than 200 km will have only a short lifetime due to atmospheric drag. We must therefore continue to depend on vertical sounding rockets for obtaining information below this altitude. As we shall see, the properties of the atmosphere between the balloon limit and 200 km are of great interest and importance. There is also a need for vertical probing out to higher altitudes so as to supplement satellite observations with spot checks on the vertical distribution of the quantity under observation.

Whereas the vertical sounding rocket will remain of great value for studies below 200 km, there is no doubt that artificial satellites are much more economical for systematic observations at greater heights. Thus a satellite circulating for a month with instruments operating continuously can give as much data as would be obtainable from several thousand vertical rocket flights using the same instruments. It follows that, whenever practicable, satellites are much to be preferred. It is not as if one reliable set of observations of the vertical distribution of some quantity were sufficient. All the quantities concerned vary with time and with geographical location, so there is a paramount need for systematic observations for which satellites are especially suitable.

With earth satellites, supplemented by vertical sounding rockets for the lower altitude studies, it is possible to meet the requirements of items (1), (2),

(3) and (4), and partially also (8). If one includes earth satellites in highly eccentric orbits, it is always possible to go far in dealing with items (5) and (6). Deep space probes are really only of major importance in connection with items (7) and (8). In the latter case they extend the range of the gravitational laboratory. From the point of view of return in the form of scientific data for a given outlay, there is no doubt that artificial earth satellites are by far the most efficient. The first step forward which renders practicable the wide range of studies under (4) is the possibility of carrying out systematic observations from outside the atmosphere. It must be remembered, however, that the minimum outlay for useful satellite launching is large compared with that involved in vertical sounding rocket flights. For this reason many countries may consider it worthwhile to carry out experiments with sounding rockets to study the atmosphere below 200 km, but not be prepared to expend the much greater resources required for the launching of artificial satellites. At a higher level of expenditure some countries may extend their activities to include artificial earth satellites but not to the relatively less effective deep space probes and so on.

Countries which do not launch artificial satellites may nevertheless participate to a significant extent in a research programme by making accurate observations by optical, radio or radar methods of the tracks of satellites launched by other countries. From such data, much of interest may be deduced about the shape of the earth and about the density of the outer atmosphere, as will be explained below.

Great as is the rate of acquisition of data by instruments in artificial satellites, there is no prospect at all that every accessible subject will have been studied exhaustively in a few years time. The new branches of astronomy which will develop are of immense scope and range and this is indeed true of most of the other subjects listed above. These general conclusions remain valid even if one were restricted to the use of comparatively close artificial earth satellites.

3.1. *Study of the high atmosphere*

A great deal of knowledge of the high atmosphere had been acquired before the last war by the use of ground-based instruments concerned with radio and acoustical probing, and with spectroscopic and magnetic observations. Two leading questions which occur when considering the value of rocket sounding and artificial satellite programmes are the following. First of all, is there much of interest to study in the tenuous atmosphere above the balloon limit ? If the answer is in the affirmative, as it certainly is, we then enquire whether it is necessary to send instruments up to make measurements *in situ*. As so much was found out before space research began, cannot we confine ourselves to use of ground-based instruments in the future ? We shall now discuss these questions.

It is true that the atmosphere, judged by ordinary standards, is very thin above 30 km altitude. Thus at this level it is about 1/100 as dense as at ground. At 100 and 200 km respectively the density is 7×10^{-7} and $3{\cdot}2 \times 10^{-10}$ times the ground value. Nevertheless, the atmosphere at these heights performs many functions of importance for life on the ground. The ozone layer which absorbs lethal ultra-violet radiation is concentrated around 20 km. Meteorites are volatilized in the neighbourhood of the 100 km level and the ionosphere, which plays an essential role in long-distance radio transmission, extends upwards from 100 km, being most concentrated at a level as high as 300 km. Short ultra-violet rays with wavelength near 600 Å cannot penetrate below about 150 km. Auroral displays occur mainly around 100 km altitude while the much weaker but uniformly distributed night airglow is generated at heights above 60 km.

The sun exerts a profound influence on the high atmosphere. Solar ultra-violet rays produce photochemical and photo-ionization phenomena which bring about great changes in atmospheric composition. Photo-dissociation of oxygen followed by consequential reactions leads to the production of ozone in relatively large concentration in a layer centred round 20 km altitude (the maximum proportion of oxygen in the form of ozone is still only about 10^{-6} at the maximum). Above 100 km, where the pressure is so low that three-body recombination reactions are very slow, the photo-dissociation of oxygen by the sunlight causes the oxygen to become mainly monatomic. Recombination processes occuring during the night provide the energy which is radiated in the form of the night airglow.

Photo-ionization by solar ultra-violet and X-rays produces the ionosphere in which there is a concentration of free electrons sufficient to affect very markedly the propagation of radiowaves of not too short wavelength. The ionization is concentrated during daytime in three layers, the E, F_1 and F_2, whose maxima are located at altitudes around 100, 200 and 300 km respectively. At night the F_1 and F_2 layers merge. In E the maximum electron concentration is about $1{\cdot}5 \times 10^5/\text{cm}^3$ rising to $2{\cdot}5 \times 10^5/\text{cm}^3$ in F_1 and $10^6/\text{cm}^3$ in F_2.

The sun and moon exert tidal influences on the high atmosphere which are of much greater amplitude than at ground. Because of these tides, the conducting ionosphere drifts across the lines of force of the earth's magnetic field. Electric currents are thereby generated in much the same way as in a dynamo. The magnetic fields due to these currents produce the steady magnetic variations observed on the ground.

All these effects arise from radiations emitted normally by the sun. Actually, however, the sun is a variable star so that other radiations are emitted from time to time when it is in a disturbed state, as indicated, for example, by the presence of sun-spots. On these occasions streams of charged particles are often emitted. The interaction of these particles with the atmosphere, in the presence of the earth's magnetic field, leads to phenomena which are mainly localized at high latitudes. These include auroral displays, magnetic and ionospheric storms. Strong correlations exist between the onset of these effects and the occurrence about a day earlier of solar flares. From this time delay the speed of the particle stream can be estimated.

This brief sketch shows the great variety of important phenomena which need to be studied. The large majority depend in some way on solar electromagnetic or particle radiation, which is absorbed in the course of producing the atmospheric effects. We can only obtain qualitative information about the radiation from ground-based observations. As it is vitally necessary to know the nature and intensity of the incoming rays before we can hope to understand the effect they produce, we have here already a sufficient justification for the use of vertical sounding rockets and artificial satellites. Without them we can never hope to have this key information. Already we know from measurements made from sounding rockets that, whereas the intensity of the solar radiation shorter than the ultra-violet cut-off at 2900 Å at first falls well below that which would be radiated from a black body at 6000°K (the temperature of the sun in the visible) the intensity radiated at soft X-ray wavelengths greatly exceeds that from such a body. It seems in fact that these X-rays may be responsible for producing the ionization in the E region. Some of the work has actually been carried out by flying spectrographs but it has mostly involved the use of luminescent phosphors and photon counters in association with suitable filters. It will be possible to put these solar observations on a systematic basis now that earth satellites are available.

During the International Geophysical Year rockets have been launched from Fort Churchill in Canada to pass through auroral displays so that the concentration and energy distribution of the charged particles, with energy greater than a few kilo electron volts, which are present can be determined. Such data will prove of great value in assisting us to understand the mechanism of excitation of auroral light.

There are many other types of upper atmospheric observation which cannot be carried out with ground-based equipment. It is very difficult to determine from radio-probing the concentration of electrons in the ionosphere above a maximum. Thus it is not possible to trace out the variation of concentration above the E layer maximum until it attains a value greater than at that maximum. Of particular interest in this connection is the way the ionization falls off in the upper part of the F_2 layer. There are many ways in which this information may be obtained by instruments carried in sounding rockets or satellites. It is possible, in fact, to derive much of interest from the analysis of the radio signals received from satellites. If the frequency is not too high these signals are modified by passage through the ionosphere. From the nature of these modifications information may be deduced about the properties of this region. Other methods employ instruments contained within the satellites which observe the electron concentration directly and these are now coming into operation. Many valuable observations have already been made from sounding rockets, some under conditions known as polar blackouts, in which ground-based radio probing is rendered impossible by strong absorption. Plans are now being made to use topside ionospheric sounding in which the signals are generated and received by apparatus in a satellite instead of on the ground.

The composition of the ions in the ionosphere is another important aspect which cannot be studied from the ground. After some unsuccessful attempts rocket-borne mass spectrographs have now provided very interesting information. They show that, up to heights of 150 km in daytime, the main positive ion is not O^+, O_2^+, N_2^+ or N^+ but NO^+. This does not mean that nitric oxide is an important atmospheric constituent at these heights. The ions first produced by photoionization are probably O^+ or O_2^+ but these are transformed to NO^+ by the reactions

$$N_2 + O^+ \rightarrow NO^+ + N,$$
$$N_2 + O_2^+ \rightarrow NO^+ + NO,$$

which will be fast enough to maintain NO^+ as the dominant ion. Observations with instruments in the third Soviet satellite have shown that, between 250 and 950 km altitude, the main ion is O^+ with N^+ present in a very much smaller proportion.

Ultra-violet absorption and mass spectrograph methods have also been used to determine the composition of the main gas at high altitudes, information again difficult to obtain, if possible at all, from the ground. It has been found that, although oxygen is mainly monatomic above the 100 km level, the proportion remaining in the diatomic form up to 150 km or so is much higher than would be possible in the absence of some mixing forces.

Apart from the applications of which examples have been described above there is another direction in which vertical sounding rockets may be used. This is essentially one of experiment rather than observation and involves deliberate modification of a region of the atmosphere by ejection of material from the rocket, followed by study from the ground of the effect produced. The first example of this was the ejection of 2 lb of vapourized sodium from a rocket at 70 km altitude at twilight. This sodium cloud was caused to fluoresce strongly by the sunlight so that the yellow glow could be observed at stations 100 miles or more from the launching point. By observing the motion of the cloud it is possible to obtain information about wind velocity and shear. Experiments are also in progress to measure temperature from the width of the emitted D lines.

Another remarkable experiment of this kind was carried out using nitric oxide as contaminant. Release above 100 km of about 5 lb of this gas produced a pale green glow due to radiation emitted in the reactions

$$\left.\begin{array}{l} O+NO \rightarrow NO_2+\text{radiation} \\ NO_2+O \rightarrow NO+O_2+\text{radiation} \end{array}\right\}$$

in which the nitric oxide acts as a catalyst for the recombination of oxygen atoms. This was a convincing demonstration of the fact that oxygen is mainly atomic at the altitudes concerned.

There is obviously great scope for this work, which has barely begun. The high atmosphere is potentially a vast photochemical laboratory free from the complication due to solid surfaces.

Even with properties which can be studied from the ground to some extent there is a real value in direct measurement. In particular, it is possible to obtain a more detailed picture of the vertical distribution. Much attention has been concentrated on the study of atmospheric structure—pressure, density, temperature and wind distribution—using various instruments in sounding rockets, and much information has been obtained up to 200 km. Here again satellites are of great value in extending the range of these observations to higher altitudes. As for the ionosphere, quite a lot can be obtained from accurate observation of the orbit of a satellite. Because of air drag the period of revolution and eccentricity decrease gradually. The rate of decrease may be related to the air density at the perigee. Much attention has been concentrated on analysis of the orbits of the first satellites with this in mind and as a result much information of interest has been forthcoming. The density at great heights is considerably higher than previously thought. It also is found to be much more sensitive to season and to solar conditions than anticipated. Improved precision can be obtained, particularly at very great altitudes, by placing inflatable balloons in orbit as their spherical shape makes it easier to relate precisely the drag force to the air density and their low density ensures a greater sensitivity to drag effects.

Although the above account of the problems and results of atmospheric physics is far from comprehensive, enough has been said to indicate the scope for space research in this field.

3.2. *Meteorological studies*

The only reason we have separated meteorological studies from the investigation of the high atmosphere is that weather conditions are mainly determined by regions of the atmosphere below that which is accessible to balloons. However, with the availability of satellites it is possible to keep the whole of the earth's atmosphere under observation so as to obtain global information about heat radiation, atmospheric motions and disturbances, cloud distribution, etc. This will provide meteorological data on a world-wide scale which should assist forecasting as well as helping to understand the details of atmospheric circulation. It may eventually make long-range forecasting practicable.

3.3. *Study of the gravitational and magnetic fields of the earth*

The actual orbit followed by an artificial satellite depends on the detailed shape and mass distribution of the earth as this in turn determines the gravitational force which is exerted on the satellite at any point of its path. If the disturbing effect of air drag can be disentangled it follows that, from precise knowledge of the orbit, we can obtain information about the figure of the earth. This has been used with success even for the second Russian satellite which was launched at a time when a world-wide network of accurate optical observing stations had not yet been set up. Thanks particularly to observations made by kinetheodolites at Ministry of Supply stations in the United Kingdom, sufficient data were obtained to show that the ellipticity of the earth derived from gravity observations was appreciably in error. If a and c are respectively the equatorial and polar radii then $(a-c)/a$ was found to be 1/298·21 instead of the previously accepted value of 1/297·1. Much more detailed information will be forthcoming in the future now that an elaborate world-wide optical tracking network exists.

One further result already obtained is that the inclinations of the orbits of the second and third Russian satellites slowly decreased, an effect which must be ascribed to forces due to high altitude winds, partly but not wholly arising from the earth's rotation.

Geodetic applications of vertical sounding rockets and satellites are being worked out and are likely to be important in the next few years.

The measurement of ambient magnetic fields from rockets, satellites and space probes is of value not only for the observation of magnetic variations at high altitudes (which helps to locate the current distribution responsible for the variations) but also for measurement of the main field. Even the secular variations of this field could be studied from satellites of long life. Two chief difficulties in this work are the perturbation due to magnetic material in the vehicle and the need for accurate determination of the satellite position at the time of each observation. Neither are insuperable, and the second does not apply to vertical sounding rockets from which measurements already taken with proton precession magnetometers have located the current system at certain latitudes which are responsible for the quiet magnetic variations.

Magnetic field measurements at great distances from the earth are important for the interpretation of magnetic storms and other effects arising from the ejection of solar particle streams. Techniques for extending the range of sensitivity are being developed.

3.4. *Astronomical observation from outside the atmosphere*

The importance of extending the wavelength range of astronomical observation is obvious—the contribution made since the war from radioastronomy, in which observations are carried out at short radio wavelengths, show, in no uncertain fashion, what can be gained. Instruments circulating in earth satellites outside the main atmosphere are capable of observing the emission from the universe in radiation of any wavelength. Ultra-violet and x-ray astronomy becomes a practical possibility and we may extend the range of radio-astronomy to longer wavelengths which cannot penetrate the ionosphere, as well as to shorter wavelengths which cannot penetrate due to absorption by atmospheric gas.

At first the techniques employed will be modest. An example of what may already be done is provided from the observations made by Friedman and his collaborators of the emission from the sky in the wavelength range 1225–1350 Å observed during a rocket flight from the U.S.A. in 1957. Relatively simple equipment was employed but the analysis of the data, which involved determination of the rocket aspect at all stages during the observations, was a formidable task. Nevertheless, the brightness distribution over a large portion of the sky was obtained and it showed regions of high intensity which did not always coincide with visible nebulosity.

It is likely that it is in the direction of astronomy that space research will make the greatest contributions. There is such a wide field for study, which will expand as technique improves, leading to stabilized platforms in satellites on which telescopes of increasing size and complexity may be mounted. No limit seems to exist to the possibilities which are opened out in this way.

3.5. *Particle radiations in interplanetary space*

It is in this direction that the most remarkable discoveries have been made through the use of artificial satellites and space probes.

In the first American Explorer satellite, van Allen introduced geiger counters to monitor the intensity of cosmic radiation. The results obtained were as expected provided the satellite was not more than a few hundred kilometres from the earth but, at greater heights, the apparatus failed to record though it continued to behave normally on return to lower altitudes. As this seemed to rule out the possibility of instrumental failure and as it was inconceivable that the cosmic ray intensity should fall to zero, van Allen turned to the only alternative—the possibility that the intensity of radiation capable of activating the counters was so great at the higher altitudes that the counters were choked. He confirmed by laboratory test that this would indeed occur if the intensity exceeded the normal value by a factor of order 10 000. Opportunity was therefore taken to include in the instrumentation of Explorer IV, launched on 26 July 1958, counters capable of handling the very high intensities expected from this interpretation. Results obtained confirmed the existence of the high intensities and provided data on the variation of the intensity with position.

Further observations were carried out with counters in deep space probes. Results from Pioneer III, launched on 6 December 1958, which travelled out to a distance of 63 500 miles from the earth, showed that there are two zones of intense radiation, an inner zone extending outwards from a few hundred miles to about 4000 miles from the earth and an outer zone extending from about 8000 miles to over 30 000 miles. Further observations with the probe Pioneer IV, which became a solar satellite, revealed a more intense outer zone—probably released particle emission from the sun. Results obtained with counters in the third Russian satellite are in general agreement with those obtained by van Allen.

It appears from the admittedly incomplete data at present available that in both zones the intensity distribution follows quite closely the magnetic lines of force of the earth. This indicates quite clearly that the radiation is composed of charged particles. At present the inner zone is considered to be relatively stable and to arise from secondary particles, produced by the interaction between cosmic rays and atmospheric nuclei, which have been shot outwards—the so-called cosmic ray albedo effect. The outer zone, on the other hand, is probably due to the emission of particle streams from the sun and is likely to vary in intensity with the state of the sun. On these views the particles in the inner zone should be quite energetic with energies of the order of 10–400 MeV whereas those in the outer zone should have much lower energies (below 1 MeV). Some evidence in support of these conclusions comes from observations taken with instruments carried in the Russian deep space probe which was launched on 22 January 1959. A very great deal must be done before the nature and origin of these radiation belts are properly understood, to say nothing of their relation to atmospheric phenomena such as aurorae and magnetic and ionospheric storms. It seems probable that the ‘ trapping ’ of charged particles in the earth's magnetic field, a possibility first discovered by Störmer as long ago as 1913, plays an important part in maintaining the zones.

3.6. *Matter in interplanetary space*

The space between the planets is no longer regarded as quite so empty as formerly. There is evidence from various sources that electron concentrations as high as $1000/\text{cm}^3$ are to be found even at distances of several earth radii. It is of much interest to study this and to determine the energy distribution of these electrons for they may well represent an outer extension of the solar corona, in which case they may possess a mean energy corresponding to a temperature as high as 200 000°K. The nature of the positive ions present is also of much interest. Presumably at some level atomic oxygen must cease to be the main constituent and be replaced by atomic hydrogen. It would be very well worth while to trace this change and to determine the degree of ionization of the hydrogen. There seems to be every prospect that instruments of sufficient sensitivity for these purposes will be available very soon.

We still know little about the concentration in space of dust particles which are too small to heat up sufficiently to vapourize on entering the atmosphere. In addition to determining the concentration of these particles it is necessary to know their energy and momentum distributions. This presents considerable problems to the instrument designer but a start is being made using impact microphones and other devices which record essentially the arrival of a fine dust particle on a sensitive area of satellite skin. More elaborate devices will be necessary to determine energy and momentum

3.7. *Lunar and planetary studies*

It would not be appropriate to enlarge here on the type of observations which would be both of scientific interest and practically feasible. There are many possibilities which we can expect will be realised, in the case of the moon, during the next few years. Before it will be possible to send probes to the near neighbourhood of Venus or Mars it will be necessary to improve the accuracy of surveying within the solar system. At present the distance to say, Venus, at any particular time is not known to better than 10 000 miles. Deep space probes suitably equipped may be used for this purpose.

3.8. *The nature of gravity*

Comparatively little is known of the nature of gravity, the weakest of the four types of force with which we are acquainted—in order of decreasing strength, the forces which hold nucleons together in atomic nuclei, electromagnetic forces, the forces which lead to beta radioactive decay and the force of gravity. In fact gravitational forces are relatively so weak that it is only because the strength of the gravitational field of a body is proportional to its mass that it becomes perceptible. We need more opportunity for experimental study of gravity to assist us in understanding its nature. With space probes available it becomes possible to extend the range of such studies by making accessible to observation the gravitational effect of massive bodies other than the earth. Furthermore, because of the cyclic character of satellite orbits and the short periods of revolution, effects can build up steadily at a constant rate per revolution to a measurable value in quite a short time. Thus it is planned to attempt the detection of the effect predicted by the General Theory of Relativity of a gravitational field on the time of a clock by comparison of the rates of highly accurate atomic clocks, one in a satellite and the other on the ground. This will be a difficult experiment but is clearly of much importance.

4. PRACTICAL APPLICATIONS OF SPACE RESEARCH

Although the most significant applications which will eventually stem from space research are likely to come from unsuspected directions, there are already a number of possible applications of artificial satellites which are being actively examined.

We have mentioned earlier the opportunities afforded for meteorological studies, not only from the research point of view but in the provision of systematic data on a global basis which can be used in weather forecasting, storm warnings, etc.

The practicability of relaying radio signals back to ground from transceivers in satellites has already been demonstrated (for example the Atlas satellite launched on 18 December 1958). With a suitable, quite limited, number of appropriately equipped satellites a world wide communication network could be set up which would be independent of the ionosphere and so would not be limited to the use of waves long enough to be reflected by it. In particular, television wavelengths could be transmitted to any distance over the earth. Even if this latter rather double-edged advantage is disregarded, there is a real chance of improving quite markedly the present channels of communication.

Satellites possessing orbits which are sufficiently far outside the atmosphere to be effectively free from air drag could be used for navigational fixes as well as for accurate geodetic measurements.

5. INTERNATIONAL CHARACTER OF SPACE RESEARCH

It does not take much thought to realize how important international cooperation is for space research, even if only to provide world wide facilities for tracking satellites and space probes and to receive the telemetred signals from them. The fact that the first satellites were launched during the International Geophysical Year in a period of highly organized international cooperation was useful in this respect. A basis for cooperation was laid and has been extended beyond the IGY through the establishment by the International Council of Scientific Unions (ICSU) of the Committee for Space Research (COSPAR) which is now taking over the responsibility for organization of international aspects of the scientific work. Other agencies are being set up to deal with the engineering side, with practical applications and with legal and other problems.

The great expense of space research is also a further compelling reason for international collaboration. At the COSPAR meeting at The Hague in March 1959, the United States delegate communicated formally the offer of space in vehicles launched by the U.S.A. for instruments designed and developed in other countries, provided the experiments were agreed, by a committee of international experts, to be suitable. The United Kingdom has taken advantage of the opportunity afforded in this way for gaining, relatively quickly, experience in the carrying out of observations from satellites. Agreement has been reached with the American National Aeronautics and Space Agency (NASA) for the launching, during the next few years, of three satellites containing all British instruments with the exception of those involved in telemetry, radio tracking and temperature control. Cooperation of this kind will certainly grow so that full advantage may be taken of scientific skills and techniques in all countries irrespective of whether a country is able or prepared to launch its own satellites.

Effort is being made to maintain cooperation on a scientific level though nevertheless paying due regard to the efforts put forward by different countries in building up the necessarily very expensive facilities required.

6. CONCLUDING REMARKS

It is hoped that the long-term nature of space research will have been made clear from what has been discussed above. Work has only just begun. An immense field of important scientific work lies before us far into the foreseeable future. Achievement on this side, as distinct from the technical, cannot be measured in terms of the distance to which a probe has been sent or by its weight. What is required is the establishment of a long-term scientific programme taking full advantage of the possibilities offered by sounding rockets and by earth satellites. Even if it were never possible to send probes to the neighbourhood of the planets or even to the moon, there would still remain the major opportunities of astronomical and geophysical research afforded by the possibility of observation from outside the atmosphere.

In contemplating such a programme we must remember that, with several circulating satellites, the amount of raw data received daily in the form of telemetry signals will be truly terrifying. The problem of analysis of these data is a very severe one and cannot be overlooked if full advantage is to be taken of the satellite techniques. There is always a tendency for analysis to lag behind observation. This would be so expensive a waste in this type of work that it must not be allowed to happen.

Diamond. Part 1

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SUMMARY

In this, the first of two articles on diamond, a review is given of its hardness, its sources, its fabrication and of many of its uses as a technological material. Some examples of etched diamond surfaces, growth features, surface polish and percussion marks are illustrated. The history of man-made diamonds is briefly reviewed.

In part 2, which will follow later, aspects to be treated will include absorption spectra, semi-conductivity, diamond counters and irradiation defects.

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1. THE HARDNESS OF DIAMOND

Carbon occurs in three distinctive forms, either amorphous, such as charcoal, or as two totally distinctive crystalline forms, the soft friable opaque shiny plates graphite, and the extremely hard shiny and transparent crystal, diamond. There is hardly a single object of modern scientific interest which has so long and so romantic a history as diamond. Indeed, records of diamond go back to Biblical times, for the diamond was stated to be one of the twelve precious stones set in the decorated breastplate of the Hebrew High Priest and Jeremiah mentions the use of the diamond as an engraving tool, so that even then it was known that diamond could be used to engrave or inscribe on hard materials.

Despite the familiar softness of carbon in its two forms, charcoal and graphite, the diamond, on the contrary, is the hardest material known to exist, and this immense hardness is of considerable technical value, so much so that diamond has strategic importance as a military material.

The early history of diamond, and also a good deal of the magical belief which surrounded this crystal, was associated essentially with its hardness. Much later it was noticed that diamond has a high reflectivity, a high refractivity and a high optical dispersion, because of which properties it could be fashioned into a flashing brilliant gemstone.

The hardness, which makes diamond unique, was noticed by the ancients. Diamond was first mentioned in Greek records in 300 B.C. being called *Adamis*, the ' unsubduable ', both because of its great hardness and also because of its reputed resistance to fire. It does indeed resist fire but not to the extent the ancients believed. The hardness resulted in the diamond acquiring a considerable ' magical ' significance. It was firmly believed that it would impart hardihood, manliness and physical strength on the wearer. It was therefore at first exclusively worn by *men*, especially warriors, and was often carried into battle.

Many curious legends arose about diamond, and hardness occupies a prominent place in these. We owe our earliest ' scientific ' knowledge about diamond to Pliny (died A.D. 79) who gravely reported many old superstitions as true. Thus he writes that if a good quality diamond be struck with an iron hammer, then the *hammer* (and not the diamond) will fly to pieces. This curious fable was accepted for many centuries, despite the fact that it is readily established that diamond, although hard, is also brittle. True, if a diamond is placed into a steel vice and this is clamped up *slowly*, then the diamond bites into the steel with ease, but if a diamond be struck a sharp blow, it breaks into pieces. It can be powdered easily with an iron pestle and mortar. The break-up of the diamond is largely due to the existence of easy cleavage direction. Here again Pliny has another strange myth, for he adds that if a diamond be dipped in fresh hot goat's blood before being struck with the hammer, then indeed now it will fly to pieces and the hammer will remain intact! It is conceivable that this story is a cover-up of a secret knowledge of the art of diamond cleavage.

The numerical hardness of diamond is difficult to assess and is complicated by the existence of more than one hardness value for the crystal. Hardness, although well understood in a general sense, is a quantity difficult to define scientifically, for one encounters a variety of kinds of hardness. The earliest scientific method for estimating hardness, due to Mohs (1822), was a scratch test. If A can scratch B then A is harder than B and a hardness scale was worked out by Mohs which is still useful. However *scratch hardness* is only one of many kinds. There is *abrasion resistance hardness*, measured by abrading off material by a known load, operating a known number of abrading strokes. Then there is *impact hardness*, measured from the rebound of a hard sphere or pendulum from the surface. The most widely used technical and engineering method involves *indentation hardness*, a measure of resistance to distortion. In this, either a ball, a cone-shaped or a pyramid-shaped diamond, is pressed under a known load into the material to be studied. If the material is soft the diamond penetrates deep and makes a large impression. If hard, the impression is small. The ' pyramid hardness ' can be computed from the ratio of the load to the area of the indentation and is arbitrarily adopted as the number of kilograms per square millimetre which the indented metal is supporting. To appreciate the hardness estimated for diamond a comparison of pyramid hardness values of other materials is useful. A typical brass alloy can have a hardness number of about 100. Steels can vary between 200 for soft steel to 800 for a hard tool steel. Silicon carbide (Carborundum), the industrial grinding material, has a pyramid hardness of about 2500. A number of borides and nitrides are a little harder than this, but not very much so.

The hardness of diamond itself is difficult to assess. This is further complicated by the fact that diamond (like most crystals) exhibits different hardness values on different crystallographic planes and in different directions on such planes. Indeed, it is only because of these differential effects that any attempt can be made to assess the hardness of diamond, for some faces are very distinctly ' softer ' than others and other ' harder ' directions can make impact pressure and abrasion effects on the softer faces. It has been found possible to indent a ' softer ' diamond face with a triangular diamond pyramid cut and polished in the ' harder ' directions. Results are uncertain, but there is little doubt that the pyramid hardness of diamond exceeds 10 000; indeed, a very much higher figure has more than once been advocated. Diamond is clearly in a class by itself from the viewpoint of hardness.

In curious contrast is the hardness of the other crystalline form of carbon, graphite, which exhibits the very low hardness figure of only 2. The hardness in diamond arises from the close-packing of the carbon atoms in the crystal lattice, which are so near to each other as to resist strongly any tendency to displacement or compression. In the graphite crystal the carbon atoms pack into tight hexagonal plates, but these are at a sufficient distance from other plates to permit of easy gliding of one plate over another, leading to a situation in which little resistance is offered to a distorting force.

In the Middle Ages ' *diamond-cut-diamond* ' was discovered. It was learnt (and kept secret by the diamond polishers' guilds) that the resistance to abrasion of diamond varies in different directions, i.e. that there are ' harder ' and ' softer ' directions. Diamond can be polished in the softer directions by using diamond powder. For some of the powder particles offer their harder directions to the surface submitted to them. The traditional lore about directional hardness has only been scientifically improved upon within recent years. Two new experimental methods have revealed precise numerical data on directional hardness. In the one, the rate of removal of material produced by a fixed load operating for a given number of strokes is measured by weighing the loss. In the other (a method used by the writer), a micro-abrasion test is used. A conical wheel is fed with diamond dust and with this the size of a small oval cut made by a fixed load operating over a fixed number of revolutions is determined. Cuts can be made in different directions and the size is a measure of the resistance to abrasion(2). Figure 1 (× 35) shows a typical such abraded surface. The volume of any cut is assessed interferometrically(6). Interference fringes are produced within the cut and these are effectively depth-contour lines, each corresponding to 1/100 000th of an inch. An example of such an

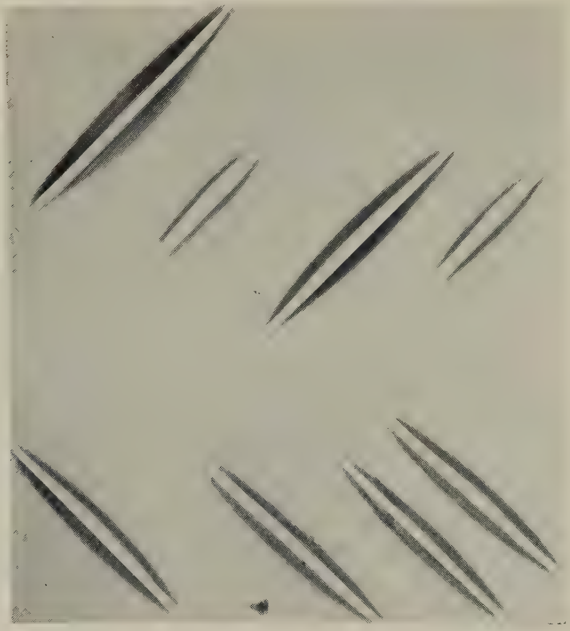

Fig. 1. Wheel abrasion of diamond surface (× 35).

interferogram is shown in fig. 2 (× 150) and from this the volume of the cut can be accurately determined.

Such studies largely confirm traditional (non-numerical) views, but further give a more precise meaning to the hardness found on different crystal faces and in different directions on each face. They have contradicted traditional views in connection with certain items of hardness.

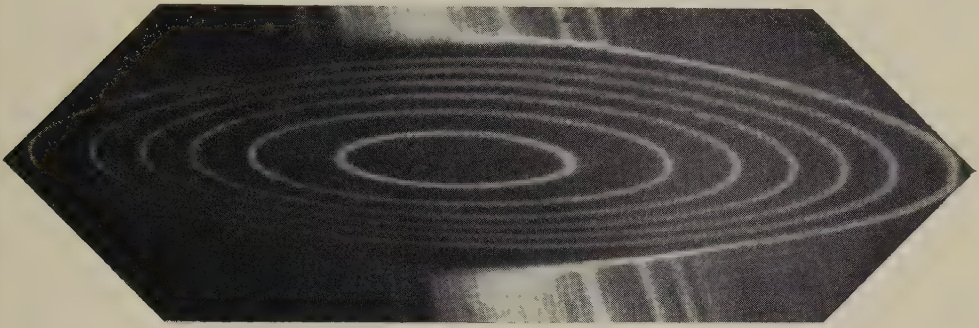

Fig. 2. Interferogram of one of the cuts of fig. 1 (× 150). This enables the volume to be computed.

2. Sources of diamond

Until the beginning of the eighteenth century all known diamonds came from one region in India, near to Hyderabad, the Golconda region. These famous ancient mines, which have yielded many great and costly gems, were practically exhausted by the late seventeenth century. Amongst their output were great gems which played a part in history as heirlooms, including the Koh-i-Noor, the Pitt, the Regent, and others. Pliny has an incredible account about early sources of diamond, stating that diamonds occur in the Orient in only one quite inaccessible valley. The local natives throw meat into the valley and the diamonds adhere to this. Eagles then swoop down and carry off the meat to their mountain nests; from which the diamonds are recovered. It is all the more remarkable a tale in that it is known that the Golconda diamond fields were at one time quite extensive and at their peak supported many thousands of workers.

The first non-Indian diamond finds were made early in the eighteenth century when extensive deposits were discovered in Brazil. Although some Brazilian diamond is of pure gem character a great deal occurs as an impure conglomerate of small black crystal masses, called *carbonado*. Here the small crystals lock each other and so resist fracture and cleavage, so that the mass is stronger than pure single diamond crystal. Its resistance to blows and impacts makes it much in demand for certain technical purposes.

It was in 1867 that a large white pebble found by a Boer child on the banks of the Orange River in South Africa was identified as a magnificent diamond and a great mining industry developed. In 1910 a further source of diamonds was found in the Belgian Congo from which indeed no less than 60 per cent of the world output now comes. Africa alone accounts for over 90 per cent of the world output of diamond. There are several minor fields in other countries.

Recently a source has been announced by Russian geologists in the Yakut region but the richness of this field has not yet been made clear.

Diamonds occur in two quite distinct ways. In South Africa they are found in deep vertical '*pipes*' of a rocky ore, the '*blue-ground*'. The great Kimberley mine is a vast pit which has been worked down to a depth of 4000 ft. Some of the pipes, such as that at the Premier mine, are 1000 yds across. There is ample geological evidence that a blue-ground pipe is a core which has been forced up from below from a great depth, from possibly hundreds of miles down. It is at the great depth where both pressure and temperature were originally very high that the carbon crystallized into diamond. Only guesses can be made as to the source of the carbon; it may have been from carbon dioxide from decomposing carbonates. The South African diamond is now extracted from the ore by crushing, washing and then flooding over rocking-tables covered with thick grease. For it has been found that diamond has the peculiar property of sticking to grease whilst the rest of the wet ore passes on.

The Congo diamond deposits produce what are called 'alluvial' diamonds (the Golconda field was of this kind too). Here the crystals are found in shallow gravels and old river beds, even in the sea near river mouths. They often show evidence of surface wear from tumbling against rock, over great periods of time. It is almost certain that these diamonds originally too came from pipes, but that these particular pipes were in past ages forced up above the surrounding level. The exposed pipes then weathered down to become the diamond-bearing gravel and sand agglomerate, on the surface, to be ultimately washed down into the present valley sites. Both types, alluvial and deep-mined diamonds, most probably then have the same deep blue-ground pipe origin according to this view. Alluvial diamonds are less easily separated by the grease table and for these an electrostatic separation technique has been developed, since diamond and the gravel constituents are differentially attracted in a strong electric field.

3. The nature of the raw material

Diamond is graded into two classes according to crystal perfection and size. There are the crystals which are pure enough and large enough to be used for jewels and there are the industrial 'stones', crystals which are either badly coloured, flawed or too small for use as gems. Diamond sizes are measured by weight, based on an ancient measure, the carat. The fairly wide-spread eastern locust-pod fruit has small round seeds which the ancients noticed were of remarkable uniformity in size. This seed was adopted in the East as a weight measure for gems and pearls and it was called the carat. After some variation the value of the carat has settled down now to be 1/5 of a gram.

The world output of diamond is now formidably large, amounting last year to 25 million carats, which, in more familiar terms, is 5 tons. Of these, 1 ton was of gem quality and found its way into jewellery (valued as raw material as 50 million pounds). The remaining 4 tons of diamond were of industrial quality (valued as raw material as 24 million pounds). Contrary to popular belief, diamonds are the commonest of all gem-stones. Part of the high price goes into the high cost of extraction, for the diamonds are thinly distributed. Five tons of ore must be raised, crushed and sorted to extract the weight of a single carat. The mines handle the formidable weight of 125 million tons of ore a year to maintain current supplies.

There is no fundamental difference between gem-stones and industrial and both are found indiscriminately mixed. The classification is merely one of purity and size. The diamond crystal is found in a variety of crystallographic forms amongst which three distinctive kinds are relatively frequent. These are (1) the octahedron, (2) the dodecahedron and (3) the flat triangular plate.

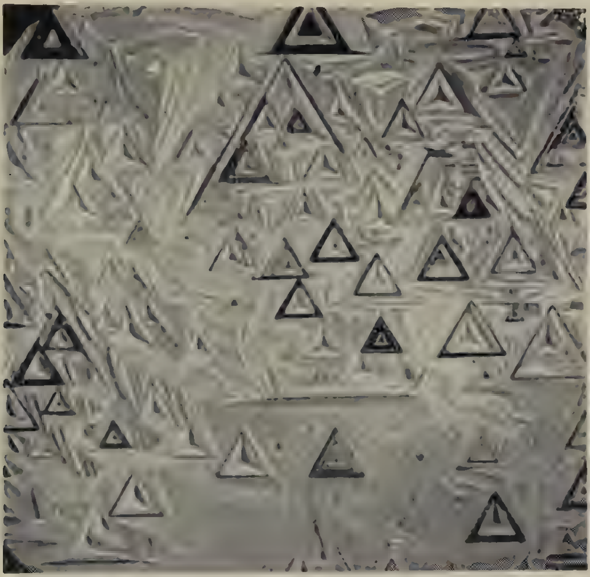

Fig. 3. Growth pits on octahedral diamond face (× 35). These depressions are called growth 'trigons'.

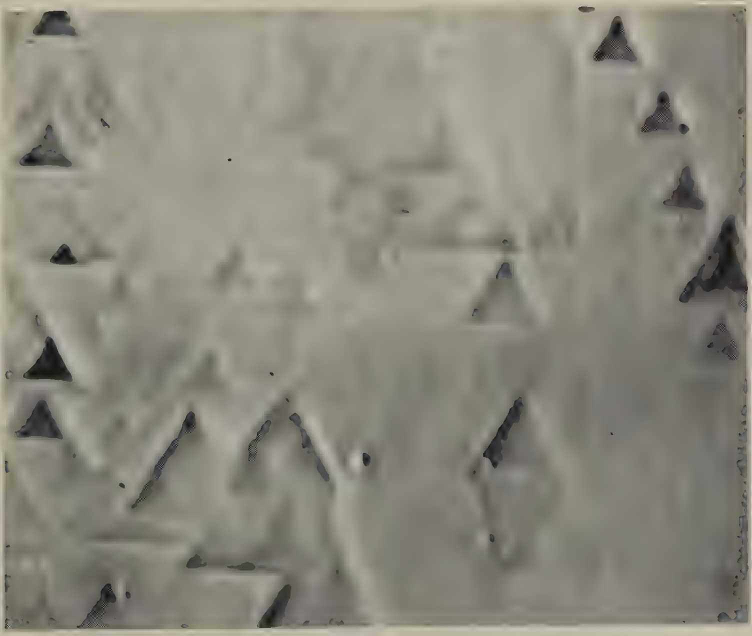

Fig. 4. Shallow growth trigons on octahedral diamond face (× 800).

Practically all octahedral diamonds exhibit a striking distribution of triangular pits which are a *growth* phenomenon. Such pits are called ' *trigons* ' and can appear in very great profusion. At times, as in fig. 3 (×35), they are easily recorded, at times, as in fig. 4 (×800), they are small and extremely shallow, often a mere few crystal lattice spaces in depth. These trigons are almost invariably regularly rectilinear and are strictly oriented, with vertex pointing to a crystal edge(1)(6).

In some mines octahedra predominate, in others the common form is the dodecahedron(6)(7). The flat triangular plate is closely related to the octahedron and often consists of a crystal twin, in which two mirror-image related crystals are combined, growing oppositely mirror-imaged about a twin plane. Twins introduce difficulties in connection with the polishing and fabrication of diamond.

Diamonds are only occasionally colourless crystals. Colours vary from a pure transparent white, through to transparent green (very rare), to blue (also very rare), then to brown and finally to a dirty opaque black. Only the transparent crystals, or those with pure transparent colouring, have value as gems. Some diamonds have a thin opaque skin and when this is abraded off a fairly clear transparent crystal is found below.

Large good quality crystals command high prices which can easily run into many thousands or even tens of thousands of pounds, and it is usually crudely estimated that the price of a good gem stone increases with the *square* of the size. Until the opening-up of the South African mines large diamonds were extremely rare and most became historical. For instance the famous Indian Koh-i-Noor diamond, which is now in the British Crown Jewels, when seen in India in 1620 by the distinguished French traveller and gem collector Tavernier, weighed then 280 carats. It has since been recut more than once and now only weighs 106 carats in its present form in the Crown Jewels. An historically famous diamond, the Orloff, now in the collection of the Russian State Jewels, and once belonging to Catherine the Great, weighs 194 carats. The Pitt diamond, which was greatly valued by Napoleon, and worn by him on the hilt of his ceremonial state sword, weighs 136 carats. Such weights are typical of the few famous large historical diamonds which came out of India. Yet from South Africa alone during the sixty years prior to 1932 (when data were made available) there had been produced no less than 1600 diamonds in the weight range 100–200 carats, 30 each exceeding 400 carats and three each heavier than 1000 carats. The most remarkable pure diamond ever found is the mighty Cullinan diamond, discovered in 1905 and weighing 3106 metric carats, i.e. over one pound and six ounces. This was cut into four large and 90 minor stones, all of which are now in the British Crown Jewels. There is no diamond in the whole of history even remotely comparable with this extraordinary gem-stone.

Whilst the Cullinan is the largest *single crystal* of diamond ever found, two carbonados (impure diamond conglomerates) have been found in Brazil of slightly greater weight. The largest of these, weighing 3148 carats was, by coincidence, also found in the year 1905.

4. The fabrication of diamond

Four distinct machining processes can be used to shape and fabricate diamonds. These are: (1) cleaving, (2) bruting, (3) sawing, (4) polishing. The need for shaping originated in the discovery in the Middle Ages that diamond could be polished into a glistening gem-stone which looked more exciting than the natural crystal, for a natural diamond is often quite an uninteresting dull pebble.

That a diamond can be cleaved seems to have been known to the early Indian lapidaries. It was known in Europe by the tenth century, although it seems to have been a lost art and rediscovered more than once after that date. With the sharp edge of another diamond a scratch can be made on a diamond edge and a smart blow with a light hammer on a blade inserted in the scratch cleaves the diamond asunder into two parts, the cleavage plane being fairly flat. Cleaving is used as a simple preliminary method for reshaping any irregularly shaped stone prior to using other more elaborate shaping techniques. It is quick and easy, but requires a very expert touch.

The original purpose of shaping the diamond gem-stone was to produce ' brilliance ' and ' fire '. It was noticed quite early in the Middle Ages that diamond has a high reflectivity (20 per cent compared with 4 per cent for glass). The high refractive index of diamond (2·42 for yellow) gives it the high reflectivity. The difference between the refractive indices for red and blue is high (0·04 as compared with 0·009 for a typical crown glass). Because of the high refractive index, light incident within the crystal striking a face at an angle exceeding $24\frac{1}{2}°$ is totally internally reflected. If the diamond can be shaped by cutting facets on it so that most of the light entering it meets another facet at an angle bigger than this total reflection angle, the light will be reflected back and out, so that the diamond becomes, in effect, as brilliant as a highly silvered mirror. Further, owing to the high dispersion, white light is broken up into its spectral colours and this constitutes the ' fire ', seen when the diamond is slightly moved.

Just when the invention of diamond polishing took place is wrapped in obscurity. Many authorities attribute the invention to Ludwig van Berghem of Bruges who described the technique in 1476. There is ample evidence however that polished diamonds were worn as jewels at least 60 years before this date and there is record of a diamond polishers' guild almost a hundred years earlier, so that the polishing art is certainly older than 1476.

The key to diamond polishing lies in the crystallographic direction hardness properties of diamond. The octahedron face is quite the hardest and cannot be abraded away or polished. But the cubic and dodecahedron faces are appreciably less resistant. If diamond (which is brittle and readily crushed) is ground down with pestle and mortar to a fine powder, the micro-fragments are still crystalline and sharp. To polish diamond this crushed powder is mixed with an oil and then applied to a (fairly porous) cast-iron wheel. This is rotated at high speed, nowadays with an electric motor, formerly by using a sufficiently big gear ratio with hand-driven belt and pulley system.

Suppose now a dodecahedron face of a diamond crystal is presented to the slurry of diamond powder and oil on the rotating wheel. Amongst the multitude of diamond particles on the wheel will be large numbers which present their hard octahedron face directions to this softer dodecahedron face. These correctly oriented particles succeed in slowly grinding away the face of the diamond. It is a laborious and time-consuming operation and the powder–oil slurry requires constant replacement. The polisher must not only know which faces to present, but on each face are relatively harder and softer directions.

It is remarkable that modern workers use practically the same methods as those used 500 years ago, power driven wheels being practically the only real major improvement introduced by modern technology.

The rate of grinding cannot be speeded up easily for if the face of the diamond becomes hot through excess applied pressure to hasten the operation, it reverts to graphite and blackens. A diamond when heated above 1200°c always reverts to graphite unless it is kept under extremely high pressure.

It used to be stated that the polished surface of a gem-stone was very flat and very smooth. Recent interferometric investigation by the author(6) shows that this is not true. Figure 8 shows an interferogram taken on the face of a diamond which has been polished with much more care than that usually devoted to gem-stones. The surface is a square of side 4 mm. Had it been smooth and flat the fringes would be straight equidistant parallel lines. There are two kinds of defect: (*a*) the surface has a distortion amounting to something like half a light wave (1/100 000 in.) and (*b*) there is a residual zig-zag microstructure which cannot be polished away. This is a crystallographic property, due to differential hardness direction. Such a surface is appreciably inferior to what is achievable on polished glass, so that tradition is wrong in this respect.

Late in the nineteenth century a method for sawing diamond was invented and this has greatly reduced the time needed for shaping diamonds. The saw is a thin strong disc of phosphor bronze clamped between much heavier discs to give it stiffness. A paste of diamond powder and oil is applied to the thin disc which is rotated at high speed. After some hours a one-carat stone can be cut through. Only certain ‘ soft ’ directions can be sawn and after sawing, according to the shape of the pieces and the final shape required, the diamond may be bruted. The bruting operation consists of mounting the diamond usually in solder, on a lathe. Whilst it is rotating another diamond is pressed against it and by this means a rough rounding is secured. Diamond machining operations lead to considerable wastage and reduction in weight but a good deal of the wastage can be recovered for making diamond powder. It is not uncommon for a crystal to lose half or more of its weight before it is polished. The optimum brilliance attainable depends upon the original shape, and the stone is polished to keep a maximum weight. To produce high brilliance a large number of facets must be polished on the stone. Various ‘ cuts ’ have been designed and as many as 56 carefully geometrically arranged faces may be polished on the one stone to secure the best brilliance and fire. Much skill is required for this.

5. Technical and scientific uses of diamond

Industrial diamond is consumed in a multitude of different ways. The more important uses can be classed as techniques using (1) single diamonds or groups of single diamonds, (2) crushed diamond powder, either loose, or bonded to a solid.

Amongst the more important uses in the first group are (*a*) drilling and boring, (*b*) lathe machine-cutting tools, (*c*) tools for truing grind-stones, (*d*) wire-drawing dies and jet apertures, (*e*) glazier’s glass-cutters.

5.1. *Drilling and boring*

Both fine and coarse diamond drills are used. There are those using single diamonds for drilling fine holes in other hard materials, such as gems, hard alloys, watch bearings, etc., and those using many for heavy drilling work. For fine work, small diamonds are mounted on the tips of needle drills, and if rotated fast, such drills can pierce the hardest of materials. More spectacular is the use of the large diamond crown-drill for rock-drilling and oil prospecting. A ring of fairly large rough unpolished diamonds is mounted round the edge of a hollow heavy steel tube and when rotated under a powerful load this can grind its way through the toughest of rocks.

5.2. *Lathe machine-cutting tools*

Diamond lathe-cutting tools are being used more and more. A diamond, some 3 mm or so long is cut and polished to a required shape, and then mounted gripped on the end of a steel shank. The hard sharp diamond cutting edge is presented as a lathe-cutting tool to the material to be worked. Such tools are particularly useful for cutting both very *soft* and very *hard* metals. For instance, the bearings of a motor-car engine can with advantage be turned from a soft aluminium–tin alloy. A superfine silky smooth finish is required and the diamond cutting tool is found to produce the smoothest finish.

At the other end of the scale of operations is the machining of the very hard tough alloys needed for the high temperatures encountered in aircraft jet engines and rocket motors. The alloys used in these constructions are too tough to be machined by ordinary steel cutting tools. In spite of the cost of repolish of fractured diamond tools their use is very economic.

5.3. *Tools for truing grindstones*

Precision grinders play an important part in engineering practice. For instance, in the aero engine many of the accurately fitting complex shapes are made by suitably shaped precision grinders with Carborundum grinding wheels of the correct exact profile. Very little wear can be tolerated and the grinders must frequently be trued back to exact dimensions. For this purpose only a diamond truing tool is adequate. Thus for such important strategic productions as aero engines, a handful of truing diamonds plays a vital part.

5.4. *Wire drawing dies and jet apertures*

Wire is made by drawing metal through a die and the wear on the die hole can be enormous. Diamond is the only really satisfactory material for wire dies. A precision hole is drilled in the diamond using diamond dust and oil on the tip of a rotating steel needle.

The same procedure is used for the manufacture of jets used for corrosive vapours and liquids. Because of the high chemical resistance, diamond jets suffer practically no wear under conditions which would severely corrode most other materials.

5.5. *Glazier's glass-cutters*

Small diamonds are extensively used for cutting sheet glass. Both polished diamonds and natural dodecahedral edges are variously used for glazier's tools.

A related use of the diamond appertaining to glass is the use of sharp diamond edges for engraving fine scales, the manufacture of optical gratings and the engraving of designs.

The second important group of uses of industrial diamond are those which employ diamond *powder*. Considerable quantities of powder are consumed and this is made by crushing inferior or small diamonds unsuited to other purposes. Large quantities of powder are used in the production of gems, and this is carefully graded as to size.

A most important and rapidly growing use is in the 'bonded' wheels which came into being some 30 years ago. In the earlier types diamond powder was mixed with a suitable resin and the mixture pressed into wheel shape. Later various metal-bonded wheels were developed, some by intimately mixing the diamond powder with a metal powder and then sintering the mixture to wheel form. Such rapid-cutting economical bonded wheels are now extensively used as saws for cutting through glass plates, or for slicing and shaping hard building stone.

Apart from the industrial uses there are numerous minor scientific uses of diamond although the quantity of material thus consumed is insignificant. Diamonds are used for measuring hardness (diamond pyramid test) for special instrument bearings, and even as counters of atomic radiations.

6. SOME PHYSICAL PROPERTIES OF DIAMOND

Diamond, being merely crystallized carbon, burns away slowly to carbon dioxide if heated in an oxidizing atmosphere, as was established even by Lavoisier. Yet a more careful study of this, recently carried out by the author, has yielded much interesting information(3)(4)(9). When heated in an oxidizing atmosphere at some 500°c very small triangular etch pits appear on octahedron faces, an example being shown in fig. 5 (× 800). These resemble growth trigons, but are *completely oppositely oriented.* Etching by this method on

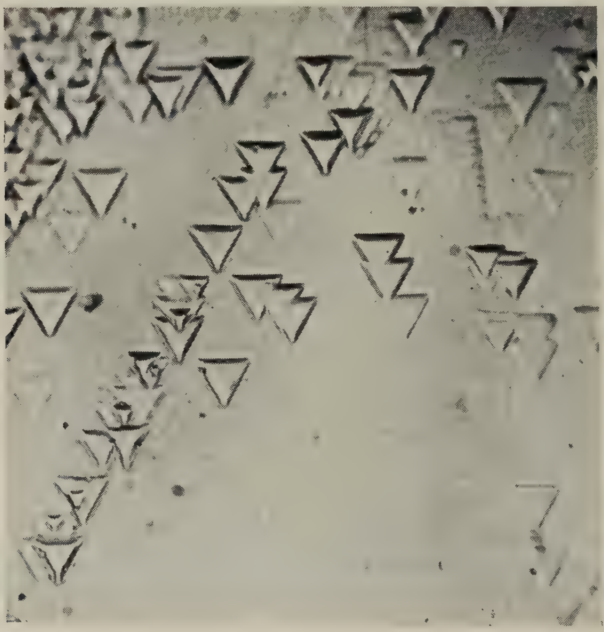

Fig. 5. Etch pits on octahedral face. These are oppositely oriented to growth trigons (× 800).

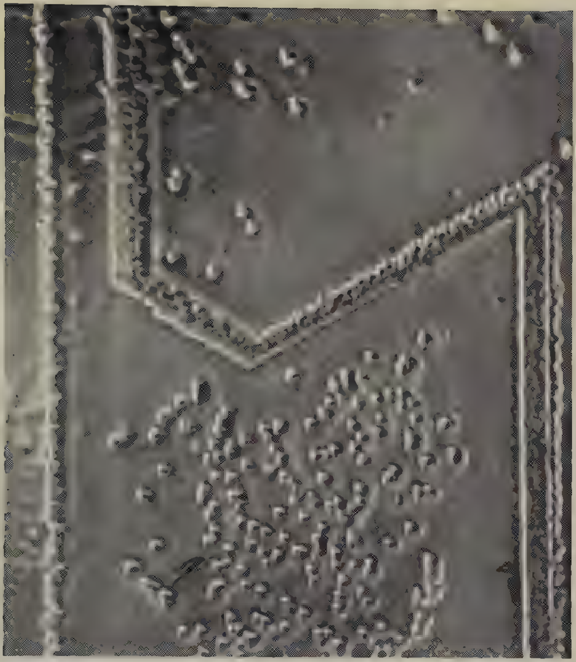

Fig. 6. Etch pits on cleaved octahedral face revealing internal stratigraphy (× 800).

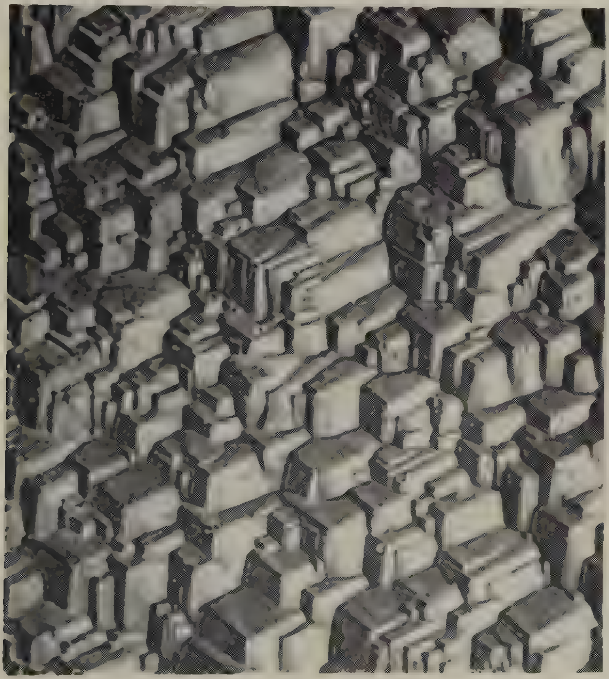

Fig. 7. Block pattern resulting from intensive etching on an octahedral face (× 800).

cleavage faces of diamond reveal striking crystallographically oriented arrays of etch pits which throw light on the internal stratigraphical structure of the diamond. Figure 6 (× 800) is a typical example.

More intensive etching can lead to a curious ' block ' pattern, on a micro-scale, and fig. 7 (× 800) shows such a block pattern resulting from the intensive etching of a diamond octahedron face(6).

Because of its low atomic weight, diamond is transparent to X-rays and this test enables spurious imitations to be detected readily. Good diamonds also transmit ultra-violet light, but it has been found that there are two distinct kinds of diamond, which have been called Type I and Type II, the latter being a little more transparent than the former. The Type II diamonds transmit somewhat further in the ultra-violet than the Type I diamonds. These two types also differ in their transparency to infra-red radiations. The differences are attributed to a sub-mosaic structure. Both types serve equally well for most purposes.

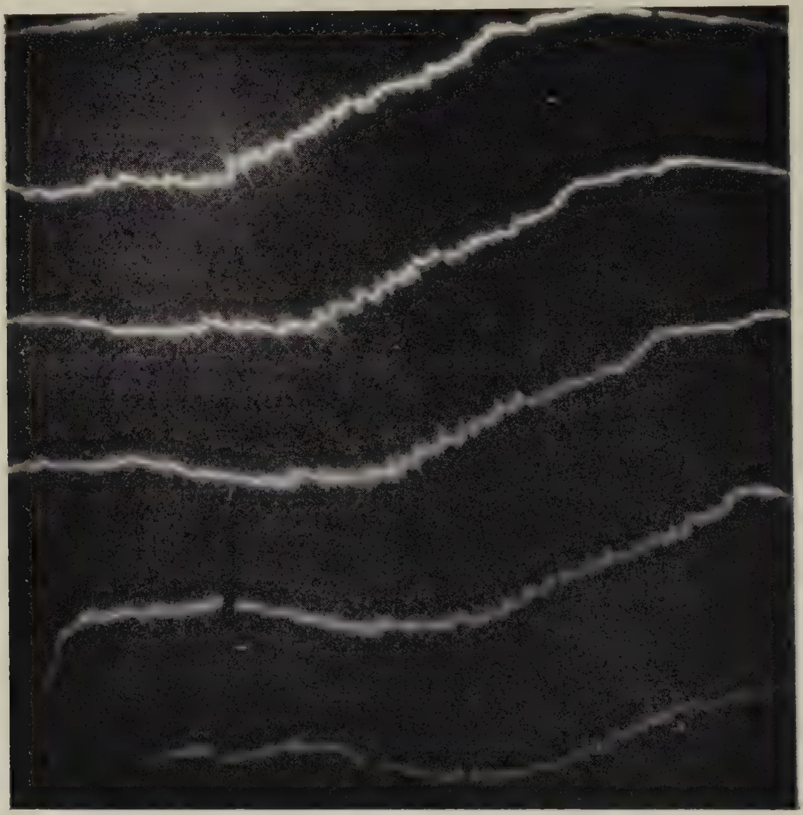

Fig. 8. Interferogram of highly polished diamond surface (4 × 4 mm).

Of the many coloured diamonds found, most of the colouring is due to the the inclusion of a minute amount of a foreign metal. Some, but not many, may have been coloured through accidental radioactive irradiation in the earth. If a colourless diamond is exposed to neutron irradiation in an atomic reactor it first turns green, later black. Bombardment of a diamond with high-energy electrons induces a blue coloration. These irradiation colouring effects are largely removed by heating the diamond to about 450°c. The irradiation produces the colour by knocking atoms out of the crystal lattice and producing strains. The heating re-anneals the strained crystal which tends to return to normal with disappearance of the colour.

When pure, diamond has a specific gravity of 3·52 but the measured value for any particular diamond depends closely upon the amount of impurity that may be included.

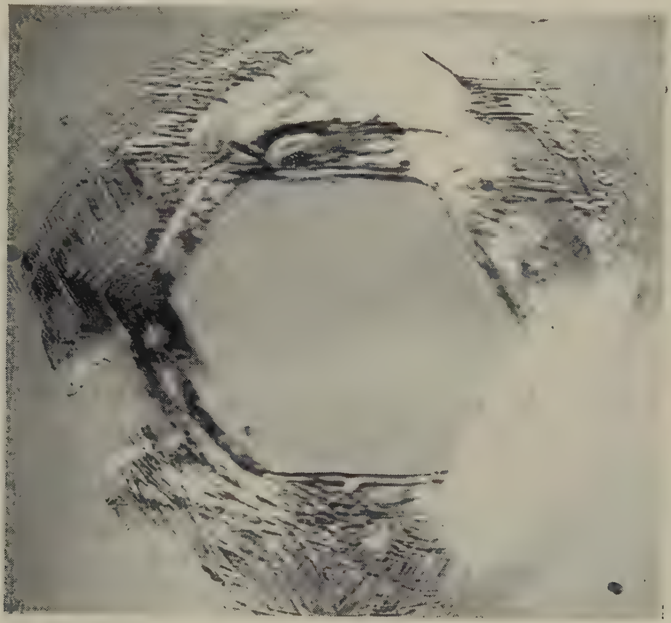

Fig. 9. Hexagonal oriented crack from tungsten carbide ball loaded with 750 tons per square inch (× 400).

An unexpected property of diamond is its thermal conductivity, which is exceptionally high for a non-metallic mineral. The thermal conductivity is 150 times that of glass. Diamond is in fact more than twice as heat-conducting as iron. It feels quite cold to the touch, just as does a metal, and this is one of the many independent tests which the expert jeweller uses to help him to recognize a diamond.

Diamond has also an abnormally low thermal coefficient of expansion, even less than that of invar steel. This property is important in connection with the use of diamond in cutting-tools and drilling crowns, where it can become hot whilst doing work.

Diamond is highly incompressible being 20 times better than quartz in this respect and of course the great hardness is closely allied to the high incompressibility.

Yet despite its great hardness, it has been shown by the author that diamond is surprisingly easily fractured to produce symmetrically shaped percussion marks. If a ball-ended diamond is pressed onto a flat octahedron face of another diamond, then with a load of a few kilograms a small hexagonal ring crack appears, of side perhaps 1/25 mm(5,8). Other materials, like tungsten carbide balls, can do the same and fig. 9 (× 400) is typical, showing a hexagon crack with surrounding deep lying damage. Measurement shows that the ring-crack was induced at a load of 750 tons per square inch, much less than expected. This happens because the diamond has cracked in the easy cleavage directions. Such studies have an important bearing on the ultimate failure of diamond machine tools.

7. MAN-MADE DIAMONDS

The attempt to make diamond artificially has a curious history. In 1828 Cagniard de la Tour claimed to have made diamond, but this cannot be substantiated. Then in 1880 Hannay declared that he had made diamonds by heating carbonaceous material together with lithium to red heat in sealed iron tubes. A high pressure developed and many tubes burst, but he claimed to have converted the carbon to diamond. His work was considered suspect by many. Prolonged repetition of Hannay's experiment by Parsons in 1920 at considerable cost, gave a negative result. Recent work shows that Hannay's temperatures and pressures must have been inadequate.

In 1894 Moissan claimed to have produced minute black diamonds by dissolving carbon in molten iron and then quenching in cold water. The sudden contraction was considered to produce a high pressure and on dissolving away the iron, small hard shiny black crystals were found. It was not established whether or not these were diamonds or only hard iron carbides or spinels. Despite a successful repetition by Crookes in 1909, Parsons remained convinced in 1920 that up to that time no synthetic diamond had been made.

Within recent years some theoretical calculations were made concerning the comparative thermodynamic stability of graphite and diamond. It transpired that graphite would probably transform to diamond if it could be kept at a temperature above 2700°C and at a pressure of at least 50 000 atmospheres. It appeared that although graphite was the stable form of carbon at low pressure, diamond was more stable at very high pressure and temperature. Further once the graphite transformed to diamond it would remain diamond when the temperature and pressure were reduced.

In 1955 a group under the leadership of F. P. Bundy, at the General Electric Laboratories in the U.S.A., reached pressures of 1 500 000 lb per square inch at a temperature of over 2700°C. This pressure corresponds to that which would be experienced at a depth of 240 miles below the surface of the earth. Such a temperature and pressure could experimentally only be held for a relatively short time in only a small volume. By the use of a carbonaceous material (details have not been released) diamonds were produced.

The diamonds so formed are small and impure and mostly of micro-size. A few relatively larger samples have been produced, the largest being 1 mm in length and this took 16 hours to grow. It may be that crushing-diamond for powder purposes might be produced at some future date at good economic rates.

That the crystals produced are indeed diamond has been adequately proved by X-ray analysis. They are very impure and certainly very far removed from natural gem-stone quality, apart from their minute size. They are only 85 per cent carbon as compared with a typical gem which is nearly 100 per cent carbon. The experiments have a very long way to go yet before diamond crystals even remotely resembling moderately sized gem-stones can be produced artificially.

The same research group have produced a new crystal compound, a cubic form of boron nitride, named borazon, which, it has been claimed is harder even than diamond. This has yet to be verified, but if it is true then borazon powder should revolutionize the polishing of diamonds, if it is obtainable in adequate amounts at economic prices.

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Some recent developments in the many-body problem

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SUMMARY

A brief account is given of the reasons why the many-body problem has recently become the subject of a rapidly expanding literature. After a survey of the main ideas of recent theories, it is shown how these ideas have played a rôle in giving us a better understanding of such topics as superconductivity, plasmas, and liquid helium. In the theory of the many-body problem the main branches of theoretical physics applied are Hamiltonian mechanics or quantum mechanics (to describe the motion of the constituent particles of the many-body system under consideration) and statistical mechanics (to describe the collective action of the large number of particles in the system). As these subjects are relatively advanced we have discussed in an appendix the main concepts of Hamiltonian mechanics and some of those of quantum statistics, including the definition and some of the properties of fermions and bosons.

1. INTRODUCTION

In the present paper we shall be concerned with one of the topics in theoretical physics which recently has come very much to the centre of attention, and which deals with problems from many branches of modern physics. As such it is one of the few meeting places for those specialists who are slowly taking over from the all-rounders in physics. Theoretical physics itself is a modern subject which was hardly recognized as a separate branch of physics until the very end of the nineteenth century. Its birth and its recent tremendous growth are consequences of the rapid development of physics, a development so fast that it is unlikely that we shall ever again witness a physicist like Fermi who could continue to excel both in theoretical and in experimental physics. Indeed, the situation is rapidly reaching a stage—long ago reached by the mathematicians—where two specialists in different branches of physics cannot discuss one another's research. This is less true in theoretical than in experimental physics, and there are still subjects such as the one under discussion where theorists in various branches can meet and use the same methods, each to solve his own particular problem. Apart from this common interest in the many-body problem to which we shall return, there have been two more stimuli. The first one is the frustration felt by a number of eminent field theorists who, looking for a branch of theoretical physics less sterile than much of modern field theory, saw that they could apply their powerful methods of analysis to the many-body problem. The second stimulus was the sudden interest in electron plasmas, awakened by the realization of their importance for the possible production of thermonuclear power. Indeed, plasmas are one type of system studied extensively in this field, others being superconductors and liquid helium, problems which have always attracted theoretical physicists. Finally, the so-called Brueckner theory of the atomic nucleus also belongs to the category of many-body problems, as well as many solid state problems.

The present paper is planned as follows. In the next section we shall discuss in some detail the present status of theoretical physics, its successes and failures, and its division into roughly two categories which for the sake of simplicity we shall call *classical* and *modern* physics. We shall then draw attention to the difficulties inherent in any system of interacting particles, leaving ourselves wondering why theoretical physics can solve any problems at all; and we shall offer an answer to that last question. In the third section we shall consider the main lines of attack used in recent many-body problem theories, while in the last section we shall discuss how these ideas have been applied to some of the problems mentioned a moment ago.

2. THE GENERAL STATE OF THEORETICAL PHYSICS

Until relatively recently physics could be divided into the following four main branches: properties of matter, heat, light, and electricity and magnetism, and even Sommerfeld's recent set of lectures on theoretical physics shows this division. This division leaves out of account practically all of what one might with some justification call 'modern physics': statistical mechanics, quantum mechanics including atomic physics, and nuclear physics. This division between what we shall call for the moment classical physics and modern physics is a very important one. It not only is a historical one, dividing those parts of physics which were studied before the middle of the nineteenth century from those in which present-day research is most active, but also it separates phenomenological physics, that is, physics of matter in bulk, from 'microscopic' physics, that is, physics where one is interested in the properties of the constituent particles themselves. Finally, it may be said that this division separates those parts of physics where theory has been successful from those where theory is still groping for an answer. Putting it differently, we can say that in classical physics one can work with those general laws of nature, of which Planck was so fond; while the specific properties of different materials should find their explanation in modern physics. The various ways of describing this dichotomy are, of course, intimately related. Natural philosophy started by describing and classifying natural phenomena. Earlier theories were essentially a way of formalizing such classifications, and the more extended the class of phenomena studied, the more general were the laws describing them, culminating in such universal laws as the first and second law of thermodynamics and Maxwell's equations for electromagnetism. The second law of thermodynamics is, indeed, a typical example of classical theoretical physics: it gives a rule rather than a theory and is of such generality that it will hardly ever lead to a numerical prediction of the possible outcome of a future experiment. Modern theoretical physics, however, is concerned with such quantitative predictions, and its numerical results are based on the numerical values of an extremely small number of basic natural constants, such as the mass and charge of an electron, Planck's quantum of action, the velocity of light, Boltzmann's constant, and so on. Even so, the frontiers of fundamental theory are pushing at diminishing the number of basic quantities, the values of which must be given *a priori.* In classical physics, however, one practically always includes in the theory a number of material constants which could be used as parameters available to produce agreement between theory and experiment.

If we accept the rather facile statement that modern physics deals with quantitative laws while classical physics contains a number of adjustable parameters, we see immediately why theoretical physics nowadays runs so often into trouble. In the present paper we shall not be concerned with a discussion of whether we know what first principles to apply. This would need a separate paper and is still far from settled. For our discussion, however, we may assume that quantum mechanics in its present form can be used as a basis for our computations. It follows immediately that for many problems ordinary classical mechanics can be used, as it is a limiting case of quantum mechanics in the limit where the quantum of action becomes negligibly small. Why do we still run into difficulties, if we know our basic equations? The reason is that practically all physical systems are many-body systems. We have now arrived at the central problem of most theoretical physics, the problem which gives the title to the present paper. Once we recognize this fact, the problem suddenly changes completely and we have to explain why we are able to solve any problems at all, and this will, indeed, be the subject of our discussions.

To see this, let us use yet another approach to theoretical physics. Newton's first and second law are completely sufficient to deal with the behaviour of a single particle, either free, or moving in a given external field of force. If the third law, *actio est reactio*, is also taken into account, we can also deal with two interacting particles in a uniform gravitational field. Since this simple case contains already the basic ideas along which many theories of many-body systems are constructed, I shall, for future reference and to remind my readers, give the treatment of this case in detail. Consider two particles of masses m_A and m_B; let $\mathbf{F}_{AB}$ and $\mathbf{F}_{BA}$ be the forces exerted, respectively by particle A on particle B and vice versa, let $\mathbf{F}_B$ and $\mathbf{F}_A$ be the forces acting upon A and B because of the common gravitational field, and let $\mathbf{x}_A$ and $\mathbf{x}_B$ be their positions (fig. 1).

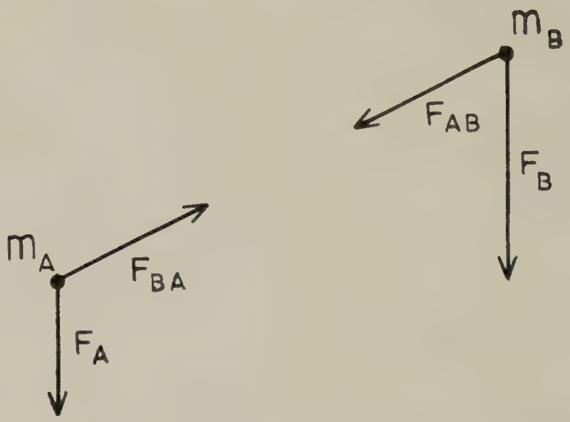

Fig. 1. Motion of two interacting particles in a common uniform gravitational field.

We shall assume that all forces are conservative. We can consequently think of the particles being in a potential field, and express the forces as the gradients of the corresponding potential energy (compare eqn. (2)). Moreover, we shall assume that the force between A and B depends on the relative position $\mathbf{x}_A - \mathbf{x}_B$ only. From Newton's third law it follows that

$$\mathbf{F}_{AB} + \mathbf{F}_{BA} = 0, \tag{1}$$

and if the forces are conservative we have further

$$\mathbf{F}_{AB} = -\mathbf{F}_{BA} = -\nabla_B U_{AB} = \nabla_A U_{AB}, \tag{2}$$

where ∇_A and ∇_B are the gradient operators with respect to $\mathbf{x}_A$ and $\mathbf{x}_B$, respectively, and where the potential energy U_{AB} is a function of $\mathbf{x}_A - \mathbf{x}_B$. Finally we have

$$\mathbf{F}_A = m_A \mathbf{g}, \qquad \mathbf{F}_B = m_B \mathbf{g}, \tag{3}$$

where $\mathbf{g}$ is the gravitational acceleration. The equations of motion for A and B are (dots indicate differentiation with respect to time)

$$m_A \ddot{\mathbf{x}}_A = -\nabla_A U_{AB} - \nabla_A U, \qquad m_B \ddot{\mathbf{x}}_B = -\nabla_B U_{AB} - \nabla_B U. \tag{4}$$

These equations are usually solved by introducing the centre-of-mass coordinates $\mathbf{X}$ and the relative coordinates $\mathbf{x}$,

$$(m_A + m_B)\mathbf{X} = m_A \mathbf{x}_A + m_B \mathbf{x}_B, \qquad \mathbf{x} = \mathbf{x}_A - \mathbf{x}_B, \tag{5}$$

and one finds from eqns. (2) to (5),

$$M\ddot{\mathbf{X}} = M\mathbf{g}, \tag{6 a}$$

$$\mu \ddot{\mathbf{x}} = -\nabla U_{AB}, \tag{6 b}$$

where ∇ is now the gradient with respect to $\mathbf{x}$, and

$$M = m_A + m_B, \qquad \mu = m_A m_B / M. \tag{7 a}$$

Both eqns. (6) are now equations of motion of a single-particle type, and can be solved by quadrature in the usual way. The single 'particles' entering into the eqns. (6) are really 'quasi-particles'; we shall return to this concept of quasi-particles in the next section (see discussion of eqn. (11)).

If we introduce one more particle into our system, we are immediately faced with an insuperable problem. The famous three-body problem was the centre of research in celestial mechanics for a long time around the turn of the century and although approximate solutions were obtained by various different methods, an exact solution could not be found. If more particles are added, the problem increases in complexity and we are faced by well-nigh unsurmountable mathematical difficulties in attempting to obtain even approximate solutions of the equations of motion. Although all we have said has been couched in the language of classical mechanics, the same conclusions hold *mutatis mutandis* for quantum mechanics.

Realizing the difficulties facing anybody who wanted to treat theoretically a system consisting of many particles, Krönig, and later on Clausius and Boltzmann, tried to make a virtue out of the difficulties and to use probability arguments. As Krönig put it in 1856 in the introduction to a paper on the behaviour of a perfect gas: "The orbit of any atom will be so irregular that it is impossible to evaluate it. According to the laws of probability one may, however, assume a complete regularity instead of this complete irregularity". This possibility of using statistical methods to treat a many-body system opened up many avenues along which progress might be made. However, it was soon realized that success was only attained in those cases where the constituents of the system did not interact, or interacted so weakly that the interaction could be treated as a small perturbation. This meant essentially that statistical mechanics can treat perfect gases, and nearly perfect gases, involving no more than the second virial coefficient. If higher virial coefficients have to be taken into account, that is, essentially, if the interaction can no longer be treated as very weak, statistical mechanics breaks down, except when we are willing to produce a phenomenological theory. Statistical thermodynamics, that is, thermodynamics approached from a statistical point of view though still a theory dealing with bulk matter of given macroscopic properties, falls into the category of successful 'classical' theories.

After this pessimistic account of theoretical physics one is tempted to ask not why theory has not been more successful, but why there have been any successes at all. If it be true that classical mechanics, or quantum mechanics, can only deal with systems of non-interacting or weakly interacting particles, while in actual physical systems the constituents will be interacting strongly, why is it possible to describe these systems? To give some more specific examples: Why is the free-electron theory of metals developed by Lorentz and Sommerfeld so successful? Why is the shell model of nuclei successful? How is it possible to predict the behaviour of liquid helium near the absolute zero? From the foregoing the answer to those questions is hardly surprising. The reason for many successes is that the problem of the behaviour of systems consisting of a large number of interacting particles can often be reduced to the mathematical problem of the behaviour of systems of a large number of non-interacting or only very weakly interacting particles. A system of non-interacting particles can be treated exactly, that is, its energy levels can be found —provided always that the corresponding one-particle problem can be solved, but this is a question of computational, not of conceptual difficulties—and from a knowledge of its energy levels one can deduce by the standard methods of statistical mechanics its equilibrium behaviour. In the next section we shall discuss various ways of introducing quasi-particles, as these entities which play the rôle of independent constituents are often called for obvious reasons.

3. COLLECTIVE BEHAVIOUR AND QUASI-PARTICLES

There are essentially two ways of reducing a many-body problem to that of a system of non-interacting or weakly interacting constituents, which seem at first sight to have very little in common. We shall discuss them separately even though the final result in both cases is very similar. The first method we shall call the effective field method or the self-consistent field method, and the second one the approach through collective coordinates. Before we do this we remind ourselves of our aim. We start with a system of N particles which we shall consider to be point particles for the sake of simplicity. We can write the energy of a particle in terms of its Cartesian position coordinates, $\mathbf{x}$, and its linear momentum, $\mathbf{p}$. If the energy is expressed in these variables it is called the Hamiltonian (compare the Appendix). The Hamiltonian, H, of the system, that is, its total energy, is of the form

$$H=H(\mathbf{x}_1, \ldots, \mathbf{x}_N, \mathbf{p}_1, \ldots, \mathbf{p}_N), \tag{7b}$$

where the $\mathbf{p}_i$ are the momenta of the particles. Our system has $3N$ degrees of freedom and we should like to find a set of coordinates $\xi_1, \ldots, \xi_{3N}$ with corresponding momenta $\pi_1, \ldots, \pi_{3N}$ such that the Hamiltonian in the new coordinates and momenta is of the form

$$H(\xi_1, \ldots, \xi_{3N};\ \pi_1, \ldots, \pi_{3N}) = \sum H_j(\xi_j, \pi_j), \tag{8}$$

where each of the $3N$ H_j depends on only one set of ξ, π: ξ_j, π_j. In many cases the H_j will not be all the same function of their arguments. If there are n

different H_j in the sum on the right-hand side of eqn. (8) the problem to which our original problem has been reduced is that of a mixture of n different kinds of non-interacting quasi-particles, that is, we have expressed the Hamiltonian for the whole system as the sum of Hamiltonians, each of which can be *regarded* as that of a single, independent particle. The reduction of the Hamiltonian (7 *b*) to (8) is the ideal to which one aspires, but usually one has to content oneself with getting instead of (8) an expression of the kind

$$H(\xi_1, \ldots, \xi_{3N};\ \pi_1, \ldots, \pi_{3N}) = \sum H_j(\xi_j, \pi_j) + H'(\xi_1, \ldots, \xi_{3N};\ \pi_1, \ldots, \pi_{3N}), \quad (9)$$

where H' cannot be written as a sum of terms each containing the ξ_j, π_j pertaining to only one degree of freedom. This term H' can be called the *interaction* term; it describes the interaction between the quasi-particles. Our problem is, however, solved approximately as long as H' can be neglected in comparison to H, at least to a first approximation. One can then first solve the independent particle problem and take the interaction into account later on, for instance, by the usual methods of perturbation theory.

We can easily see how the example of a two-body system discussed in the previous section fits in with this programme. Equation (6 *a*) can be derived by the usual Hamiltonian formalism. The corresponding Hamiltonian is in this case

$$H_{\text{com}} = \mathbf{P}^2/2M + (\mathbf{g}\,.\,\mathbf{X}), \quad (10)$$

where $\mathbf{P}$ is the momentum conjugate to $\mathbf{X}$, while eqn. (6 *b*) corresponds to a Hamiltonian

$$H_{\text{rel}} = \mathbf{p}^2/2\mu + U_{\text{AB}}(\mathbf{x}), \quad (11)$$

where $\mathbf{p}$ is conjugate to $\mathbf{x}$. We see here the appearance of two quasi-particles, one with a mass M moving in a potential $(\mathbf{g}\,.\,\mathbf{X})$ and one with a mass μ in a potential $U_{\text{AB}}(\mathbf{x})$. This example is not completely academic as the electron–hole pairs which are called *excitons* are of this kind (*vide infra*). In this case each quasi-particle corresponds to three degrees of freedom like a real particle, but this is by no means necessary and we shall later on meet with quasi-particles which correspond to, for instance, only one degree of freedom.

We must now discuss how the reduction of the Hamiltonian (7 *b*) to (8) can be performed, using effective field methods. After that we shall discuss the collective behaviour approach. In the effective field approach we shall still distinguish two cases. The first one arises in the so-called Hartree–Fock or self-consistent field method for atoms and in the Brueckner approach to nuclei. It is used when we are dealing with interacting particles of one kind. The second one arises especially in the case of solids where one is dealing with one kind of particle, usually the electrons, moving in the potential field produced by other particles, usually the heavy ions. It leads to the so-called effective mass approach.

In the following we shall make the following simplifying assumption. We shall assume that the Hamiltonian of our N-body system is of the form

$$H = \sum_j (\mathbf{p}_j^2/2m_j) + \sum_j U(\mathbf{x}_j) + \tfrac{1}{2}\sum_{j \neq n} U_{jn}(\mathbf{x}_j, \mathbf{x}_n), \quad (12)$$

where both j and n run from 1 to N, m_j is the mass of the jth particle, $U(\mathbf{x})$ is the common potential in which all particles move (often just the potential containing the system within a given volume V) and the U_{jn} are the interaction potentials between two particles. Equation (12) pre-supposes that all interactions in the system are the result of a superposition of two-body interactions. This assumption is to a high degree of approximation justified for most systems, but may be wrong for the nucleons inside a nucleus.

From eqn. (12) we see that each particle moves in a field which is the sum of an external field, $U(\mathbf{x})$, and of the field produced by all the other particles. The idea of the self-consistent field method is to replace the second field by a smoothed-out field, $V(\mathbf{x})$ say. The Hamiltonian (12) is thus replaced by a Hamiltonian H' given by the equation

$$H' = \sum_j H_j, \quad H_j = \mathbf{p}_j^2/2m_j + U(\mathbf{x}_j) + V_j(\mathbf{x}_j). \tag{13}$$

The motion of each particle, which is governed by the Hamiltonians H_j, can in principle be solved, Once all orbits are known we can by time averaging evaluate the effective field in which the different particles are moving. If that field is the same as the one assumed in eqn. (13) the problem is solved. If it is not the same, we adjust the V_j until the solution is self-consistent. This method is usually applied in quantum mechanics, where instead of a time averaging we can use the ordinary quantum mechanical averaging. We must draw attention to the fact that the effective field acting upon the jth particle will depend on the states in which the other particles are, and this method is thus mainly of interest for finding the ground state and perhaps a few of the excited states of the system, but will be difficult to use to find the thermodynamic behaviour of a system.

The effective-mass approach is one used to eliminate the interactions between the electrons in a solid and the periodic field produced by the heavy ions. In that case the Hamiltonian is of the form (12), where the common potential U is a periodic one. One can show that for many purposes the Hamiltonian can be replaced by the following one

$$H' = \sum_j (\mathbf{p}_j^2/2\mu') + \tfrac{1}{2}\sum_j U_{jn}(\mathbf{x}_j, \mathbf{x}_n), \tag{14}$$

or even by

$$H'' = \sum_j (\mathbf{p}_j^2/2\mu), \tag{15}$$

where the μ' and μ are so-called effective masses.

It is probably a good thing to discuss in somewhat more detail the physical background of eqns. (14) and (15). In eqn. (15) we are clearly dealing with free quasi-particles. The original electrons of mass m which interacted both with the ions in the lattice and with one another have been replaced by quasi-particles of mass μ. This effective mass is often quite different from the electron mass m and can even be negative, in which case our quasi-particles are the so-called holes which play such an important part in semiconductor theory. The difference between m and μ is caused by the interactions and one sometimes talks about a bare particle of mass m, and a clothed particle of mass μ. Put differently, the bare electron is surrounded by an ‘interacting cloud’ which means that its response to an accelerating force is changed. This in turn means, if we use the language of Newton’s second law, that its inertial mass is changed: it needs a different force to produce the same acceleration. This difference between the bare mass and the clothed mass explained the Lamb–Retherford shift in the spectrum of atomic hydrogen. The electron of the hydrogen atom is in that case interacting with the electromagnetic field and this interaction leads as in the case of solids which we discussed just now to a change of mass, or as it is sometimes put, to a *renormalization* of the mass. The renormalization not only of the masses of elementary particles, but also of their charges and magnetic moments has been one of the main objects of post-war field theory.

The subject of mass- and charge-renormalization is a very difficult one and really falls outside the scope of the present paper. It is complicated, moreover, by the occurrence of infinite expressions. The fact that such infinities occur clearly points to our nearly complete lack of understanding of the foundations on which modern physics is based. In essence, however, the change in the mass of the electron is brought about by a process similar to the one producing the difference between μ and m mentioned a moment ago. We may also mention that the Lamb-shift refers to a difference of two energy terms, both of which are infinite but in such a way that if one considers suitable limiting processes, the two infinities cancel, leaving a finite result which agrees remarkably well with experimental data.

In the effective-field methods one approaches the many-body problem from the point of view of the original particles. Incorporating interactions either in the potential energy (self-consistent field methods) or in the kinetic energy (effective-mass methods) the resultant Hamiltonian is of a suitably simplified type. The collective approach, on the other hand, considers possible motions of the system as a whole, that is, collective motions, and introduces for each collective degree of freedom a collective coordinate ξ_j (from now on we shall reserve ξ and π for collective coordinates and their conjugate momenta and use x and p for ‘particle’ coordinates and momenta). There are usually less collective degrees of freedom and the result is therefore that the final form of the Hamiltonian is

$$H = H_{\mathrm{coll}} + H_{\mathrm{part}} + H_{\mathrm{int}}, \tag{16}$$

where

$$H_{\mathrm{coll}} = H_{\mathrm{coll}}(\xi, \pi) \tag{17}$$

is the collective Hamiltonian depending on the collective degrees of freedom only,

$$H_{\mathrm{part}} = H_{\mathrm{part}}(x, p) \tag{18}$$

is the ‘particle’ Hamiltonian depending on the ‘particle’ degrees of freedom only, while H_{int} depends on both the ξ, π and the x, p. If the collective approach is to be successful, H_{int} must be negligibly small, while H_{coll} and H_{part} must be reducible to a sum of Hamiltonians depending on one degree of freedom only. We must draw attention to the fact that if there are n collective degrees of freedom, the particle Hamiltonian H_{part} will correspond to only $3N-n$ degrees of freedom as compared to the $3N$ degrees of freedom of the original Hamiltonian. This difficulty is sometimes circumvented by introducing redundant variables or subsidiary conditions. In those cases H_{part} corresponds apparently to $3N$ degrees of freedom—the advantage being that it can be symmetric in the N particles, or quasi-particles, which are closely related to the original particles —but in fact there are n relations between the $3N$ particle coordinates so that only $3N-n$ of them are really independent. Another way to avoid this difficulty is to introduce as many collective degrees of freedom as there are degrees of freedom, an approach used with some success by Yevick and Percus. We may add that in many discussions about the collective behaviour of many-body systems the problem of how to treat H_{part} is not considered at all.

As the collective coordinates are usually functions of the original Cartesian position coordinates of the N particles, H_{coll} will contain the π_j in the form of a homogeneous quadratic expression, and we shall therefore try to obtain H_{coll} in the form

$$H_{\text{coll}} = \sum_j \tfrac{1}{2} A_j \pi_j^2 + \sum_j f_j(\xi_j), \tag{19}$$

where one hopes that the A_j are constants while in many cases one can find such ξ_j that

$$f_j = \tfrac{1}{2} B_j \xi_j^2, \tag{20}$$

in which case H_{coll} is a sum of one-dimensional harmonic oscillator Hamiltonians. In this case one may say that the quasi-particles of the problem are the particles with Hamiltonian $\frac{1}{2}A_j\pi_j^2 + f_j(\xi_j)$, that is, particles with an effective mass A_j moving in the potential f_j. In many cases it is, however, more profitable to introduce second quantization and to use the quanta (energy packets $h\nu_j$, where $(2\pi\nu_j)^2 = B_jA_j$) as the quasi-particles of our system. This is, for instance, usually done in the case of crystal vibrations where the quasi-particles are the phonons.

In the next section we shall consider several cases where this method of introducing quasi-particles through a collective approach has been successful. Let us, however, at this point once again consider the two-body system of the previous section. Equation (10) gives clearly the 'collective' Hamiltonian of the centre-of-mass motion, while eqn. (11) gives the 'particle' Hamiltonian of a (quasi-) particle with the reduced mass μ as its mass. We may add that the three degrees of freedom corresponding to the motion of the centre-of-mass are the most easily treated collective motions. In the case of a free system both the f_j of eqn. (19) and H_{int} of eqn. (16) vanish, but the difficulty of the redundant variables or the subsidiary conditions is a real one.

We shall not discuss here how one can find the ξ_j once one has an idea of the kind of collective motion which the N-body system can show, but only mention that Tomonaga has developed a very elegant method to do this. We shall quote in the next section several results of applying the collective approach. Before doing this we must, however, discuss one more important point. We have shown how in many cases the many-body problem can be reduced to the problem of a system of non-interacting quasi-particles. If we want to evaluate the properties of this system, we need to know the statistics of the quasi-particles. Although at first sight this seems a difficult problem to treat rigorously, one often has some idea of the statistics of the quasi-particles involved. For instance, one would expect the 'quasi-electrons' in a solid to be fermions and the phonons to be bosons. How complicated the situation often is can be seen from the fact that a system of spin $\frac{1}{2}$ particles, that is, of fermions, can give rise to spin waves (*vide infra*) the quanta of which are bosons. Bogolyubov has developed an elegant way to introduce quasi-particles in such a way that their statistics are known from the beginning. His idea is to introduce the collective coordinates through a canonical transformation from the Cartesian coordinates of the original constituent particles. In that case, one can easily evaluate the basic commutators which determine the statistics of the quasi-particles. We must refer to the literature given at the end of this paper for a discussion of Bogolyubov's method (especially references 3, 16, 17, and 18).

4. APPLICATIONS

In this section we shall discuss briefly (i) the application of the self-consistent field method to atoms and nuclei; (ii) some aspects of solid state many-body problems, namely, excitons, polarons, and spin waves; (iii) collective motion in electron plasmas; (iv) collective motion in nuclei; (v) liquid helium; and (vi) superconductivity. Requirements both of space and of level make it impossible to do anything but give a very sketchy account of recent developments in these fields. The reader should, however, get some impression from this discussion of the wide-spread use of recent developments in the many-body problem.

4.1. *The atomic and nuclear shell model*

The fact that there exist closed shell atoms and nuclei is usually rather glibly attributed to the Pauli principle. The electrons in the case of the atoms are, it is said, moving in a common central field, produced by the nucleus and they will fill up the lowest available energy levels. In a central field the main separation of the energy levels will be the one corresponding to different values of the principal quantum number, while the separation due to a difference between the azimuthal quantum numbers is much smaller. A closed shell will thus occur when all levels corresponding to a given principal quantum number are filled up. This looks, indeed, to be the case for He, and Ne, but A shows a closed shell behaviour, even though its M-shell (the third shell) is not yet filled. This discrepancy can easily be patched up. The $4s$ level is lower than the $3d$ level because a $4s$-electron comes nearer the nucleus and penetrates thus into the charge clouds of the $1s$, $2s$, $2p$, $3s$, and $3p$ electrons; it thus feels on the average a stronger attractive force and this is sufficient to more than counteract the advantage of a 3-quantum state over a 4-quantum state. The concept of penetrating orbits played an important and elucidating part in the old quantum theory and made it possible to understand qualitatively not only the electronic configurations of the ground state of most elements in the periodic system, but also to understand many features of atomic spectra. There is, however, an important inconsistency in the picture as it is usually presented. In taking penetrating orbits into account we are no longer considering non-interacting particles in a common central field, but particles whose interaction is important. Indeed, the atomic spectra can only be understood, if we assume the electrons to be added one after another to the bare nucleus. The first electron moves in the full nuclear attractive field, thus screening the nucleus partly from the second electron, and so on. In that case, however, the energy levels do not correspond to the same central field, and the basis for an application of the Pauli principle seems no longer to be present. The answer to this apparent paradox is provided by the Hartree–Fock self-consistent field method. The problem of the nucleus with its Z electrons is tackled along the lines sketched in the previous section. There is, however, one difference, namely, that we are dealing with a quantum mechanical problem, and we should perhaps briefly indicate how the self-consistent field method works in this case.

It is well known that the simplest way to go over from classical mechanics to wave mechanics is by changing the Hamiltonians into operators which act upon wave functions. These wave functions describe the state of the system, just as the particle orbits do in classical mechanics. If the Hamiltonian operator of the total system is the sum of operators referring to one particle only, the wave function Ψ of the system can be written as the product of single particle wave functions, or as a sum of such products. If the Hamiltonian operator corresponding to the classical Hamiltonian (12) can be replaced by the operator corresponding to the Hamiltonian (13) we can use the approach through single particle wave functions. These wave functions determine in the case of charged particles such as the atomic electrons a charge density in which the other electrons move. After trial and error one-electron wave functions ϕ_j are found for each of the electrons which are such that all ϕ_j except one, say ϕ_{j_0}, together produce a charge density which will lead to a potential V_{j_0} to be inserted into eqn. (13). The last wave function ϕ_{j_0} is then a solution of the Schrödinger equation with the Hamiltonian H_{j_0} of eqn. (13). The total wave function of the system Ψ will then be a so-called Slater determinant,

$$\Psi = \begin{vmatrix} \phi_1(\mathbf{x}_1) & \phi_2(\mathbf{x}_1) & \cdots\cdots & \phi_Z(\mathbf{x}_1) \\ \phi_1(\mathbf{x}_2) & \phi_2(\mathbf{x}_2) & \cdots\cdots & \phi_Z(\mathbf{x}_2) \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ \phi_1(\mathbf{x}_z) & \phi_2(\mathbf{x}_Z) & \cdots\cdots & \phi_Z(\mathbf{x}_z) \end{vmatrix} . \tag{21}$$

The wave function Ψ of (21) is a sum of products of single particle wave functions. The Pauli principle is here satisfied in its original form, namely, by leading to a completely antisymmetric wave function, that is, to a wave function which changes its sign whenever the coordinates of two electrons are interchanged. This can be seen to follow from the elementary property of determinants that they change sign whenever two rows are interchanged. The reason why the earlier approach led qualitatively to the right results is that the wave functions $\phi_1, \phi_2, \ldots, \phi_Z$ qualitatively are of the same shape as the ones guessed at from a glib application of the Pauli principle in its usually quoted form.

In the case of an atom the attractive field of the nucleus keeps the electrons in the neighbourhood of the nucleus, even though their mutual interactions are repulsive. In the case of a nucleus, however, the nucleons, that is, the protons and neutrons making up the nucleus, do not experience a common external field of force, but their mutual interactions are attractive. The self-consistent field method can again be applied, albeit somewhat changed. It would lead us far too far afield if we tried in this paper to describe the so-called Brueckner method. As the shell model of the nucleus has been so successful in explaining the nuclear properties (see the article by Blin-Stoyle in the preceding issue of this journal) it is, perhaps, useful to discuss briefly in how far the Brueckner theory differs from the Hartree–Fock method. The first difference is that instead of considering one particle in the smoothed out field of the other $N-1$ particles, one considers a pair of interacting particles in the smoothed out field of the other $N-2$ particles. As far as that is concerned, the Brueckner theory is an improvement on the usual self-consistent field method. However, due to the particular character of the nuclear forces, the original problem cannot be solved, and one solves instead by self-consistent field methods a model problem. This model problem is chosen in such a way that (i) it can be solved by the modified Hartree–Fock method, and (ii) it resembles as closely as possible the original problem. The model many-nucleon wave function is one of the form (21), that is, one corresponding to a shell model. Unfortunately, the actual wave function bears little resemblance to the model wave function so that a justification of the complete shell-model picture cannot be given. However, it turns out that the energy levels given by the model show probably nearly the same spectrum, that is, the same appearance of gaps, as the real energy levels, and this explains the existence of stable closed-shell configurations, the so-called magic number configurations which are an essential feature of the nuclear shell model (see again Blin-Stoyle's article).

4.2. *Solid state many-body problems*

The most extensively studied and probably the best understood many-body problem is that of lattice vibrations. Instead of describing the motion of the ions in a crystalline lattice by their position coordinates, we introduce the so-called normal coordinates of classical mechanics. It is well known from classical mechanics that the Hamiltonian of a system of $3N$ degrees of freedom can be written in the form (19) with the f_j given by (20) in the small amplitude approximation, provided the ξ_j are the normal coordinates. This means that the behaviour of a crystalline lattice is formally equivalent to the behaviour of $3N$ linear harmonic oscillators. These oscillators could be used as the quasi-particles of the system, but one usually chooses instead of them the lattice vibration quanta, the so-called phonons. Their Hamiltonian is of the form (15), that is, they form a system of non-interacting particles. This concept of a 'gas' of phonons has turned out to be extremely fruitful and many properties of solids especially at low temperatures such as their specific heat (Debye T^3-law) and their thermal conductivity can readily be explained using this concept.

A second kind of solid-state quasi-particles was mentioned in the previous section. They are the 'quasi-electrons' and the holes, that is, fermions with a mass different from the free electron mass. We saw that this effective mass was a consequence of the periodic field, produced by the ions, in which the electrons move. Another consequence of the periodic field is the appearance of so-called energy bands; this corresponds to an energy spectrum where values of energies which can occur, the so-called allowed energy bands, alternate with energy values which cannot occur, the so-called forbidden energy bands. This spectrum is a one-particle energy spectrum, that is, it gives the possible energy values for the (quasi-) particles. The physical reason for the occurrence of these bands is the same as the occurrence of Bragg-angles for the reflection of x-rays from a crystal lattice. If the electron possesses a certain energy which, according to de Broglie's hypotheses, corresponds to a certain wavelength it may undergo total reflection: the electron cannot move freely through the crystal and we are in a forbidden energy band.

As the electrons (and the quasi-electrons and holes) are fermions they will fill up all available lowest energy levels. In a metal the last allowed energy level occupied will be next to another allowed energy level which is empty, and it needs only an infinitesimal amount of energy to excite the electrons in the highest occupied levels (fig. 2*a*). This means that even the smallest electrical field can produce a current: we are dealing with a conductor. In a semiconductor or an insulator, however, the highest occupied energy level occurs at the top of an allowed band and will be separated from the next allowed level by an energy gap:

the width of the forbidden band (fig. 2*b*). It thus takes a finite amount of energy to excite an electron, and conduction is not so easily produced. The difference between a semiconductor and an insulator is a quantitative one, and depends on the actual width of the energy gap; if it is narrow we are dealing with a semiconductor where ordinary thermal agitation will lift some electrons across the gap, while in the case of a wide gap, even that mechanism of excitation is absent and we are dealing with an insulator.

Let us consider a moment the first excited state of a semiconductor. We have just seen that the ground state will be one where all allowed energy bands up to a certain energy are fully occupied and all allowed bands above that energy completely empty. The first excited state looks thus to be a state where one electron from the top of the last occupied band is lifted to the bottom of the first empty allowed band, leaving a hole in the occupied band (fig. 2*c*). This hole behaves like the holes mentioned in the previous section, that is, it behaves like an electron with a negative mass. Looking at Newton's second law this means that it will move against an electric field which would accelerate an ordinary, free electron. The same result would have been obtained from a particle with a positive mass, but a charge opposite to that of an electron. One can, indeed, show that for all intents and purposes one can describe a hole as being a particle with a positive mass and a positive charge, like the positron in Dirac's hole theory. If that is the case, one would expect the hole, left by the electron in the filled band, to interact with the electron and to form a hydrogen-like structure with its own energy level scheme. Since a hydrogen-like structure has negative, discrete energy levels, one would expect the lowest excited state to have an energy which is less than the gap-width above the ground state energy. This is, indeed, found to be the case. In semiconductors *excitons*, as this electron–hole combination is called, exist. They can be described by equations similar to (6). The equation corresponding to (6*a*) will now describe the motion of the entire exciton through the lattice, while in eqn. (6*b*) U_{AB} is the Coulomb potential between a positive and a negative charge. The experimental evidence about excitons fully supports this picture.

Another solid-state quasi-particle is the *polaron*. If an electron moves through a crystalline lattice it will interact with the ions and they will be displaced from their equilibrium positions. A displacement of charged particles in a dielectric medium such as a crystal means the setting up of a polarization in the medium. This polarization will be such that it will try to trap the electron. In other words, the crystal with the electron in it will adjust itself in such a way that its energy is lowered by producing a polarization field which, on the one hand, is held in place by the electron and on the other hand traps the electron. While the electron is moving through the lattice it is carrying along a cloud of polarization. The combination electron–polarization cloud is a typical quasi-particle and it will possess its own effective mass, the value of which will depend on the dielectric properties of the medium. These polarons will occur in polar crystals such as rock salt and their properties have been studied extensively both experimentally and theoretically.

The last example of a solid-state quasi-particle which we want to mention briefly is that of spin-waves. Let us consider a system of spin $\frac{1}{2}$ particles, that is, of particles whose magnetic moment has only two possible ways of orientation, say either in the direction of the positive z-axis or in the direction of the negative

z-axis. If the interaction between the particles is such that it is energetically favourable for them to be parallel, the ground state of such a system will be one where all spins are parallel, corresponding to a maximum magnetic moment of the total system. The problem now is to find the excited states of this many-body system. One sees easily that the first excited state will be one with one spin in the wrong direction. Let ϕ_i denote the function describing the system with the

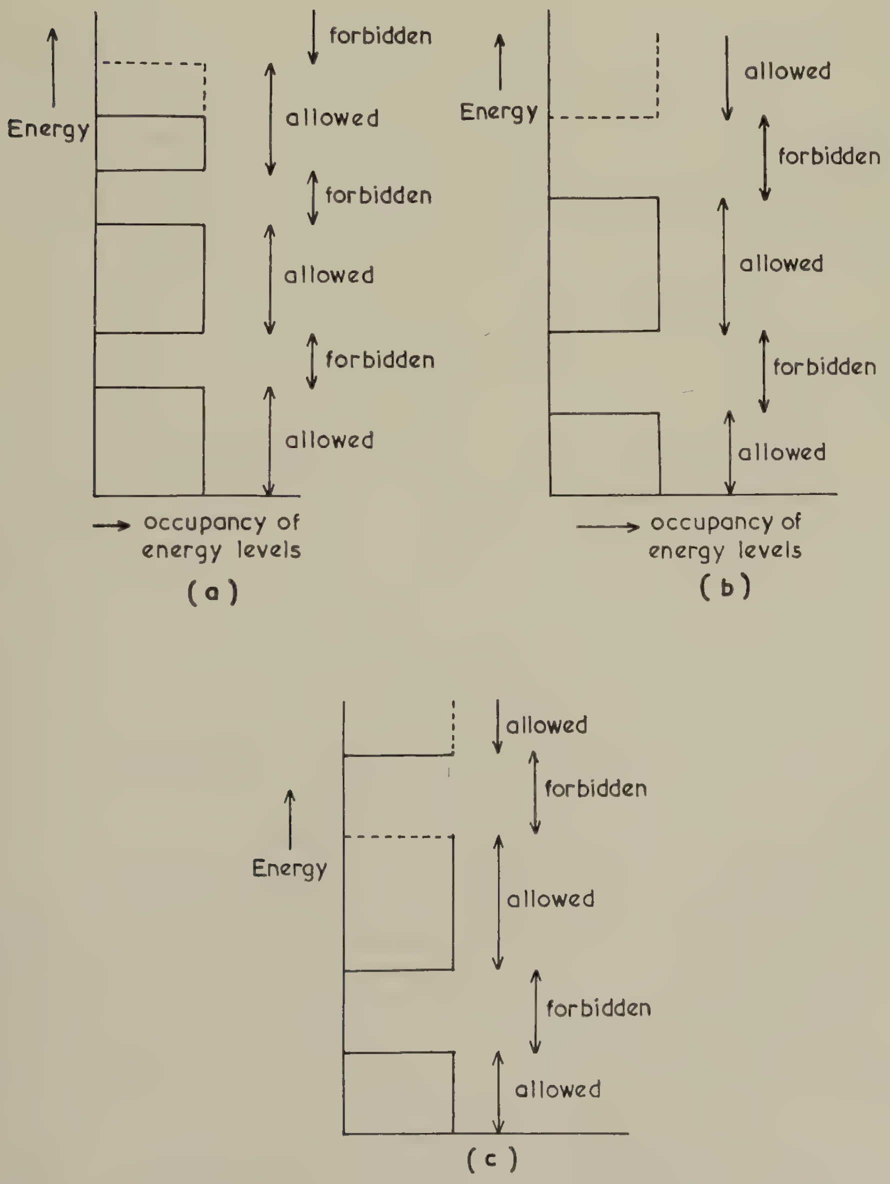

Fig. 2. Band structure of solids. (*a*) metals; (*b*) ground state of semiconductor or an insulator; (*c*) first excited state of a semiconductor, where one electron at the top of the last filled band has left it to go to the bottom of the first empty band. Full drawn lines correspond to occupied levels and dotted lines to empty ones.

*i*th spin pointed wrongly. The ϕ_i are not eigenfunctions of the system, but eigenfunctions Φ of the system will be of the form

$$\Phi = \sum_i c_i \phi_i, \tag{22}$$

where the summation is over all spins and where the c_i are coefficients to be determined from the condition that Φ be an eigenfunction. The problem is a straightforward one and one finds that the eigenfunctions Φ are those combinations of the ϕ_i where the c_i vary sinusoidally through the lattice. Physically this means that the Φ describe 'waves' where the amplitude of the 'wrongness' of the spins varies sinusoidally throughout the lattice (fig. 3 *c*). Hence the name *spin waves*. We must emphasize that all Φ describe states with one wrong spin, but the 'wrongness' is statistically distributed over the lattice instead of being localized at one definite spin. The different eigenfunctions differ by the wavelength of the corresponding spin wave. The spin waves can be considered to be quasi-particles just as the lattice waves mentioned on p. 123. In the latter case one could describe the state of the lattice by a superposition of a number of lattice waves; similarly one can describe the magnetic state of our system of spins by giving the number of different quasi-particles, that is of spin waves of different wavelength, present. A state with two wrong spins will be described by the superposition of two spin waves, and so on. It turns out that to a good approximation spin waves are independent quasi-particles and the theory can thus relatively easily be constructed; especially at low temperatures it describes the behaviour of some ferromagnetics quite satisfactorily. We may mention in particular the success of the spin wave theory to explain neutron diffraction experiments.

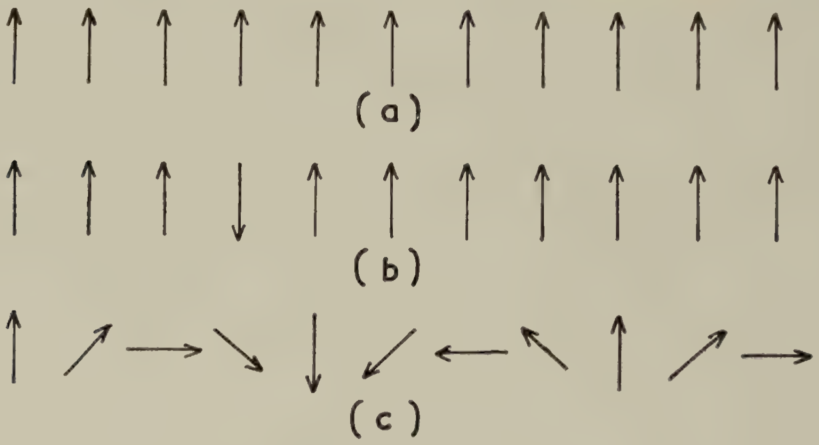

Fig. 3. Spin waves. (*a*) ground state; (*b*) state corresponding to ϕ_i; (*c*) state corresponding to Φ.

4.3. *Electron plasmas*

Because of possible applications in the field of thermonuclear research an extensive literature has recently grown up about the behaviour of a gas of electrons. We shall assume, as is usually done, that there is a smoothed out positive charge density so that the total system is neutral. Recent work on this system has used powerful field theory methods, but we shall only be concerned here with describing a few of the general aspects of the collective behaviour of this system, the so-called plasma oscillations. This is a case where one has a fair idea of what the collective behaviour looks like and where one thus uses the collective approach as described in the previous section. It turns out that the Hamiltonian is of the form (16) with the redundant variables difficulties inherent in it. The collective Hamiltonian is of the form (19) and (20) and the ξ_j are the Fourier components of the electron density. Put less technically, the collective degrees of freedom correspond to sound waves, that is, to waves of larger and smaller density moving through the electron system.

The reason why collective behaviour occurs in this case is the long range of the Coulomb forces between the electrons. The thermal motion of the electrons will try to disrupt any collective motion, but if the system under consideration is sufficiently large the long-range Coulomb forces will dominate. Indeed, if we consider a system of linear dimensions R, the total potential energy, which measures the tendency towards collective motion, will be proportional to the square of the number of particles in the system as in each interaction two particles are involved and inversely proportional to R because the Coulomb potential falls off in that way. This means that the total potential energy is proportional to R^5. The total kinetic energy, that is, the total disruptive power, will be directly proportional to the numbers of particles in the system, that is, to R^3. We see thus that for sufficiently large R the collective tendency will dominate. We also can now understand that the plasma oscillations will be waves with wavelengths limited from below. The limiting wavelength λ_D is the so-called Debye length. From our discussion we see that the Debye length measures essentially the range of the disruptive random kinetic motion; put differently, it puts a lower limit on the distance over which correlations can be measured, that is, the distance over which collective effects can be felt.

The particle Hamiltonian in (16) will, of course, no longer contain the full long-range Coulomb interactions, as these are taken into account in producing H_{coll}. From the above argument it follows that one might expect that only that part of the interaction is left which corresponds to wavelengths less than λ_D. Indeed, a careful analysis shows that the quasi-particles corresponding to H_{part} interact not through a Coulomb potential which behaves as r^{-1} when the two particles are at distances r from one another, but through a short-range potential $r^{-1}\exp(-r/\lambda_D)$, that is, a potential with a range equal to the Debye length. The total system is now described by two kinds of quasi-particles; the first kind is the one mentioned just now and the second kind is that corresponding to the plasma waves. These latter are called *plasmons* and are of the same kind as the phonons and the spin-waves.

4.4. *Collective behaviour of nuclei*

Apart from the centre-of-mass motion which is relatively more important in nuclei because of the relatively small number of nucleons in each nucleus, there are two other kinds of collective motion shown by nuclei. The first kind is that of the rotational motion mentioned by Blin-Stoyle in his article on nuclear structure and the second kind is that of the surface oscillations.

The last class of collective motion again falls very easily into our description of the collective approach. If for the sake of simplicity we start with a spherical nucleus as the equilibrium ground state, excited states can be obtained by considering deformations of the sphere (fig. 4). Treating the nucleus as a liquid drop—this effectively determines to what nuclei this discussion can be applied—one expands an arbitrary deformation in terms of spherical harmonics. As the spherical harmonics are the suitable functions to use, it follows that their coefficients in the expansion of the deformation are suitable collective coordinates, as they describe the 'strength' of each particular form of deformation, that is, of each particular collective motion. Using those coordinates one is led, in a more or less straightforward manner, to a collective Hamiltonian of the form (19), (20), a Hamiltonian which was shown by Bohr and Mottelson to be suitable for explaining certain aspects of nuclear behaviour. In the case of the rotational states one can proceed along similar lines.

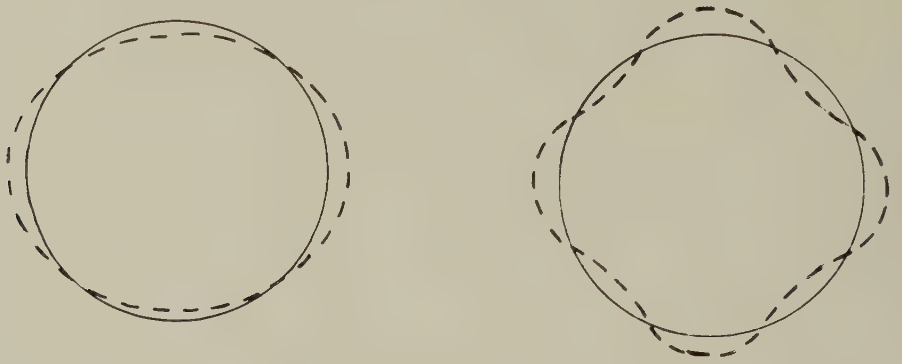

Fig. 4. Two possible excitations (dotted lines) of a spherical nucleus.

4.5. *Liquid helium*

There are essentially two problems about liquid helium which have exercised theorists for a long time. The first one is its behaviour near the absolute zero where it shows superfluidity, that is, a vanishing resistance to flow. The second one is the phase transition at 2·2°K, the so-called lambda-transition. As the forces between two helium atoms are extremely weak it was expected by some people that a perfect Bose–Einstein gas would bear a close resemblance to liquid helium and that the so-called Einstein condensation of such a gas would be related to the lambda-transition. Whether or not this is the case is still a moot point and we shall restrict our present discussion to some aspects of the behaviour of liquid helium near the absolute zero, where recent theoretical developments have been more successful.

At very low temperatures liquid helium shows two kinds of collective motion: the hydrodynamic superfluid motion and the phonons, which are sound waves. At slightly higher temperatures other collective motions, the rotons and Onsager–Feynman vortices come into play, but we shall not discuss them here. The phonons are a type of quasi-particle which we have met with several times. They are sound waves like the lattice vibrations or the plasmons and the theoretical treatment is in each case the same.

To explain the existence of superfluidity we first of all note that the reason why an ordinary fluid is slowed down is because we can slow down each of its constituent particles separately. What we are saying is the following. Consider a fluid moving at a velocity V with respect to a wall. It will take only an infinitesimal amount of energy to slow one of the particles in the fluid down from V to $V-dV$, or, in other words, there are states of the fluid, where most of the fluid is still moving with a velocity V but one particle is moving with a lesser velocity, which have an energy which is only infinitesimally larger than the energy of the original state. If this is the case we can slow the fluid down gradually. Consider now liquid helium moving at a velocity V. In this case it will cost a finite amount of energy to slow down one atom and there are therefore no neighbouring states through which we can pass in our attempt to slow down the whole of the fluid. Indeed, one can show that the only states which have an energy close to the energy of the moving fluid are those differing from the original state by a collective motion. We must emphasize that, of course, the state where the liquid helium is at rest is a state of lower energy, but it cannot be reached from the state of uniform motion through neighbouring states differing in the movement of only one atom except if there is enough energy available to bridge the one-atom energy gap. Superfluidity will thus appear if the only low-lying excited levels correspond to collective motion while the levels corresponding to a one-particle excitation are separated from the ground state by a finite gap.

If it is true that the only low-lying excited levels are phonon levels we should expect a T^3-behaviour of the specific heats as in the case of the lattice vibration and this is, indeed, found experimentally below 0·6°K. Above that temperature other excitations begin to play a rôle and the T^3-law is no longer obeyed.

We have not discussed here in how far the fact that ^{4}He atoms are bosons plays a rôle. We may just mention that it is important in proving that the energy spectrum is of the required form. Moreover, if we consider ^{3}He, we find a totally different behaviour. This is a fermion system and shows no superfluidity. Methods developed for discussing an electron gas have also been applied successfully to ^{3}He.

4.6. *Superconductivity*

As in the case of liquid helium, we are faced with the problem of explaining superfluid behaviour. We saw in § 4.5 that this is possible, provided there exists an energy gap in the one-particle spectrum. In the case of superconductivity this gap will occur for the excitation of one quasi-particle. We are dealing here with a system of fermions, the electrons in a metal—or rather the quasi-electrons as we can as before take the periodic potential produced by the lattice into account by assigning an effective mass to the electrons.

Experiments show that the temperature below which superconductivity occurs, the so-called critical temperature, varies as the inverse square root of the isotopic mass, if one considers different isotopes of the same superconducting element. From this it follows that the interaction between the electrons and the lattice will play an important rôle. If two electrons have practically the same energy, the interaction of the first one with the lattice will produce lattice waves which are more or less in resonance with the motion of the second electron, thus producing an attractive interaction between the two electrons. Such an interaction with a field (in this case the phonon field) as an intermediary is a common phenomenon in physics. Indeed, the ordinary Coulomb interaction has the electromagnetic field as intermediary and nuclear forces the mesonic field, the quanta of which are the pions. If the attractive interaction overbalances the repulsive Coulomb interaction between the electrons, there will be a tendency to form pairs of electrons. These pairs will be bosons, as they consist of an even number of fermions, and will be the quasi-particles of the superconductivity problem; considerations similar to the ones discussed for the case of liquid helium can then be used. One would expect once again an energy spectrum where the ground state is followed by low-lying excited states which all correspond to collective motions. A state with a definite velocity V for the whole system can therefore not slow down by the excitation (or de-excitation) of a single particle. The existence of an energy gap has been proved experimentally both by direct measurements of the absorption of radiation in the far infra-red as well as by measuring the temperature dependence of the specific heat which is determined by the shape of the energy spectrum. Reasonable agreement with theoretical predictions has been found in practically all cases.

Finally we note that the criterion for the occurrence of a superconducting state is the predominance of the attractive interaction due to electron–phonon interactions over the ordinary Coulomb repulsion (which we may consider to be screened if we take plasma oscillations into account). It has been shown by Pines that this criterion agrees roughly with the division between superconducting and non-superconducting metals found experimentally.

5. CONCLUSION

The subject of the many-body problem is a complex one and in a paper of this kind we can only sketch developments. Our aim has been to give some idea of the kind of approach used in this field and of the kind of system to which these ideas have been applied. In conclusion we give in a table a list of all the quasi-particles mentioned.

Quasi-particle	Page where mentioned	Brief description
Total mass of system	115	Particle considered in calculating centre-of-mass motion
Reduced mass	117	Particle considered in calculating relative motion
Conduction electron	124	Electron surrounded by ‘ cloud of interaction ’ with lattice
Hole	123	Conduction electron with a negative effective mass
Electron	121	Electron surrounded by ‘ cloud of interaction ’ with electromagnetic field
Phonon	123, 128	Lattice vibration, sound wave
Exciton	124	Electron-hole pair which is bound together
Polaron	124	Conduction electron surrounded by a ‘ polarization ’ cloud
Spin-wave	126	State in a ferromagnetic in which a ‘ wrong ’ spin is distributed over the lattice
Plasmon	127	Sound wave in a system of charged particles (i.e. in a plasma)
Roton	128	Excitation in liquid helium

List of quasi-particles

Finally we give a list of references.

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2. ter Haar, D., 1958, *Introduction to the Physics of Many-body Systems* (New York, London: Interscience). General methods and applications to the Hartree–Fock discussion of atoms, Brueckner theory of nuclei, excitons, polarons, sound waves, plasma oscillations, nuclear collective behaviour, and liquid helium.

3. *The Many-body Problem*, 1959 (Methuen). Lecture notes of a course given at Les Houches, 1958. A galaxy of authors describes at a high level their own contributions to the theory of plasma oscillations, liquid helium, nuclear behaviour, superconductivity, and ^{3}He.

4. *Proceedings of the Conference on the Many-body Problem held at Stevens Institute*, January 1957 (Interscience) (to be published). Similar to the previous reference.

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Reference 15, and also:

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6. APPENDIX

6.1. *Hamiltonian mechanics*

In the main body of the text we have used the concepts ‘ Hamiltonian ’, ‘ conjugate momenta ’, ‘ canonically conjugate momenta ’, and so on. Here we wish to remind our readers how for systems where the forces between the particles can be derived from a potential energy—so-called conservative systems—the Hamiltonian or canonical equations of motion follow from Newton’s laws.

The motion of a system of particles will be completely determined if we know the positions $\mathbf{x}_i$ of all the particles at any time. As Newton's equations of motion are second order in the time, it follows from the ordinary theory of differential equations, that the behaviour of the $\mathbf{x}_i$ is fixed, once we have given as boundary conditions the values of the $\mathbf{x}_i$ and of the $\dot{\mathbf{x}}_i$ (the dots indicate derivatives with respect to time so that the $\dot{\mathbf{x}}_i$ are the velocities) at one particular time.

It is often convenient to introduce instead of the $\mathbf{x}_i$ a set of s parameters q_k which are functions of the $\mathbf{x}_i$ and which also completely determine the motion of the systems. We need as many of these as there are degrees of freedom (s). One can then prove (see any textbook of classical mechanics) that Newton's laws of motion are equivalent to Lagrange's equations of motion

$$\left.\begin{aligned} &\frac{d}{dt}\frac{\partial L}{\partial \dot{q}_k} - \frac{\partial L}{\partial q_k} = 0, \\ &L(\dot{q}_k, q_k) = T(\dot{q}_k, q_k) - U(q_k), \end{aligned}\right\} \tag{A 1}$$

where T is the total kinetic energy, which is a function of the *generalized coordinates*, as the q_k are called, and of the *generalized velocities*, $\dot{q}_k$, while U is the total potential energy which depends on the q_k only. The function L is called the *Lagrangian* of the system.

Instead of the q_k and $\dot{q}_k$ one can also use as parameters to describe the system the q_k and the so-called *generalized momenta* p_k which are defined by

$$p_k = \frac{\partial L}{\partial \dot{q}_k}. \tag{A 2}$$

From the definition of the p_k and the laws of motion (A 1) one finds the following equations of motion for the p_k and q_k:

$$\dot{p}_k = -\frac{\partial H}{\partial q_k}, \quad \dot{q}_k = \frac{\partial H}{\partial p_k}, \quad H = T + U, \tag{A 3}$$

where H is the so-called *Hamiltonian* of the system which is equal to the total energy of the system.

Equations (A 3) are called the *Hamiltonian* or *canonical* equations of motion and the p_k introduced by eqn. (A 2) are called the momenta *conjugate* or *canonically conjugate* to q_k. We note that if the q_k are the same as the $\mathbf{x}_i$, the $\dot{q}_k$ are the $\dot{\mathbf{x}}_i$ and the p_k the linear momenta $m\dot{\mathbf{x}}_i$. If one of the q_k is an angle, the conjugate p_k is the corresponding angular momentum.

6.2. *Quantum statistics*

In statistical mechanics one uses the fact, that in most physical systems there are a great number of particles, to derive the properties of matter in bulk. If one is considering a system of independent identical particles, the first problem to be solved is to find how many different arrangements of the individual particles can lead to the same situation of the system as a whole. In the case of many identical and independent particles any arrangement will be characterized by giving the quantum states in which each individual particle finds itself, while the situation of the system as a whole will be characterized by stating how many of the particles are in each of the quantum states available to individual particles. One of the basic assumptions of quantum statistics is now that all allowable arrangements have the same weight. One can thus obtain the weight of any state of the whole systems by evaluating the number of arrangements which will lead to this state.

To find out what arrangements are allowable, it is more convenient to consider the wave function of the system than the quantum numbers which characterize it. In nature there occur two kinds of particles. The first kind, the so-called *bosons* are those for which the wave function of the whole system is completely *symmetrical*, that is, will remain unchanged, if one interchanges the coordinates of any two particles in the system. The name boson derives from the fact that the kind of statistical mechanics to which these particles give rise was first studied by Bose. Soon afterwards Einstein also considered this case and one usually talks of Bose–Einstein statistics. One can show, quite generally, that particles with an integral spin are bosons. The earliest example is that of light quanta (the case studied by Bose); other examples are the π-mesons or pions and helium atoms.

Particles of the other kind are called *fermions*. In their case the wave function of the total system is completely *antisymmetric*, that is, it will change its sign when one interchanges the coordinates of any two particles in the system. The statistics of these particles were studied by Fermi and Dirac and are thus called the Fermi–Dirac statistics. They are the particles with half odd-integer spin; examples are electrons, protons, and μ-mesons or muons. The fact that the wave function of a system of fermions is antisymmetric is sometimes expressed by saying that fermions obey the *Pauli principle* or the exclusion principle. If the fermions are non-interacting, one can easily convince oneself, that the antisymmetry of the wave function is equivalent to requiring that no two particles are in the same quantum state. This property is often, rather too glibly and slightly misleadingly, taken as a definition of the Pauli principle, but it only applies to a system of non-interacting particles.

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Electromagnetism as a second-order effect

(I) The definition of the ampere

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SUMMARY

By considering simple examples from the viewpoint of both classical electromagnetism and the special theory of relativity, it is shown that the magnetic forces between moving charges are of order v^2/c^2 times the electric forces between the charges, where v is the velocity of the charges. The relevance of electric forces in the definition of the ampere is discussed.

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1. INTRODUCTION

There is a tendency in schools at present to teach less electrostatics. To some extent this is quite reasonable as the main direct application of Coulomb's law for electrostatic charges is in atomic structure and the practical applications of electrostatic principles are confined largely to capacitors, which are considered primarily as circuit elements in a.c. theory. On the other hand, there is no occasion for students to realize that magnetic forces between moving charges are very much smaller in magnitude than the electrostatic forces. In fact electromagnetism is a second-order effect which gives rise to forces of magnitude $\sim v^2/c^2$ times the electric forces present and appears when there is *relative motion between charges and the observer.* At a recent poll, final-year physics undergraduates were asked whether two equal positive electrostatic 'point' charges moving side by side in the same direction parallel to the X axis with uniform velocity attract or repel one another (cf. fig. 5 *b*). In effect this amounts to asking whether the electric force of repulsion or the magnetic force of attraction predominates. Thirty per cent said they attract, thirty-five per cent said they repelled and thirty-five per cent did not know.

The ratio of the electric to magnetic forces in a simple case can be calculated using formulae familiar to most VIth form students. The Lorentz force on a charge q moving with velocity $\mathbf{v}$ in an electric field $\mathbf{E}$ and a magnetic field $\mathbf{B}$ is given by $q\mathbf{E}+q/c\,.\,\mathbf{v}\times\mathbf{B}$, where $\mathbf{E}$ and q are expressed in e.s.u., $\mathbf{B}$ is in gauss and $\mathbf{v}$ and c in cm sec^{-1}. The first term will be called the electric force and is independent of the velocity of the charge whilst the second term, which depends on the velocity, will be called the magnetic force. Absolute c.g.s. units are generally used in the text as the author feels that, at present at least, most readers will be more familiar with orders of magnitude in c.g.s. units rather than in m.k.s. units.

2. FORCES BETWEEN TWO PARALLEL CONVECTION CURRENTS

Consider two infinitely long, straight, thin, uniformly charged, non-conducting wires *in vacuo* lying in the XY plane and moving in the positive X direction with uniform velocity v relative to an observer at rest at the origin (fig. 1). Let λ be the electrostatic charge per centimetre length of the wires, in e.s.u. per cm, measured by the observer at rest at the origin. We are assuming very thin wires so that polarization effects in the wires can be neglected.

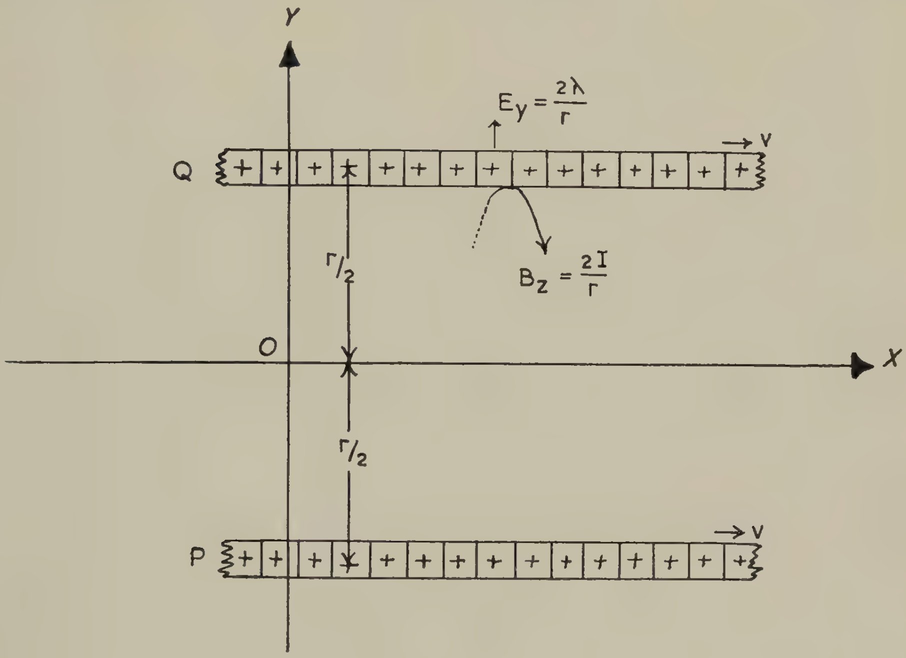

Fig. 1. The forces between two convection currents.

Application of Gauss's law and Ampère's circuital law gives the electric and magnetic fields at a distance r cm from the wire P.

We have

$$E_y = \frac{2\lambda}{r} \tag{2.1}$$

where λ is in e.s.u. and

$$B_z = \frac{2I}{r} \tag{2.2}$$

where I is in e.m.u. Now

$$I\ (\text{e.m.u.}) = \frac{\lambda\ (\text{e.s.u.})}{c} \cdot v \tag{2.3}$$

where c is the ratio of the unit charge in e.m.u. to e.s.u. which equals the velocity of light.

The electric field gives rise to a repulsion in the Y direction of $2(\lambda^2/r)$ dynes per cm length of the wire Q.

The magnetic field gives rise to an attraction in the Y direction of $2I^2/r$ dynes per cm length of Q, which is $2 . \lambda^2 . v^2/(r . c^2)$ dynes per cm length.

The resultant force on unit length of Q is a *repulsion* of

$$\frac{2\lambda^2}{r}\left\{1 - \frac{v^2}{c^2}\right\} \text{ dynes per centimetre length.} \tag{2.4}$$

In this simple case the ratio of the magnetic force to the electric force is v^2/c^2 where v is the velocity of the charges *relative to the observer.*

Using m.k.s. units, if σ is the charge in coulombs/metre, R is the separation in metres and v and c are measured in metres/second then

$$\text{Electric force} = \frac{\sigma^2}{2 \,.\, \pi \,.\, \epsilon_0 \,.\, R} \text{ newtons/metre;}$$

$$\text{Magnetic force} = \frac{\mu_0 \,.\, \sigma^2 \,.\, v^2}{2\pi R} \text{ newtons/metre.}$$

The ratio of the magnetic force to the electric force is equal to $\mu_0 \,.\, \epsilon_0 v^2 = v^2/c^2$, since $c = 1/\sqrt{(\epsilon_0 \mu_0)}$.

When a current is flowing in a conductor, the electrons may be assumed to be travelling with 'drift velocity' whose mean value will be of the order 10^{-2} cm sec^{-1} for currents of about an ampere in the ordinary sort of wire. If $\bar{v}$ is the mean drift velocity, n the number of charge carriers (conduction electrons) per cm^3, a the area of cross section of the wire in cm^2, and q the charge on each carrier in coulombs, then the current is $na\bar{v}q$ amp. In a typical case, for 1 amp flowing in a copper wire of cross section 10^{-2} cm^2, taking n to be $8{\cdot}5 \times 10^{22}$ cm^{-3}, and q to be $1{\cdot}6 \times 10^{-19}$ coulombs, we find $\bar{v}$ to be about $7{\cdot}3 \times 10^{-3}$ cm sec^{-1}. Hence the ratio of the magnetic forces between the moving electrons in such a current to the electric forces between them is $\sim 10^{-25}$. However, in a copper conductor there is no resultant free electric charge as the charges on the moving electrons are neutralized by the positively charged copper ions which are virtually at rest in a stationary conductor. For an electrically neutral conductor the electric forces between the moving electrons are compensated by the electric forces due to the positive ions and the resultant force is merely the relatively small magnetic force between the moving charges.

3. THE DEFINITION OF THE AMPERE

The new definition of the ampere, as laid down by the International Electrotechnical Commission in 1948 is not in itself associated with the trend towards the use of m.k.s. units. It would be simple enough to record it in centimetres and dynes. It merely prescribes that the 'weighing' operation by which electrical units are linked to existing mechanical units shall be that of finding a *force* between suitable current-carrying conductors instead of that of finding a *mass* of silver liberated in electrolysis, and was introduced because it allowed higher accuracy in standardizing work. In this, the ampere is defined as "That unvarying current which, if present in each of two infinitely thin parallel *conductors* of infinite length and 1 metre apart in empty space, causes each conductor to experience a force of exactly 2×10^{-7} newtons per metre of length". This is illustrated in fig. 2*a*. In this case the electric forces between the moving electrons are neutralized by the presence of positive ions, leaving a resultant magnetic force of 2×10^{-7} newtons per metre length.

Now, moving electrostatic charges constitute 'convection' currents. If we had two infinitely long thin charged non-conductors 1 metre apart in empty space each having a charge of 10 000 coulombs per metre and both moving in the same direction relative to an observer at a uniform velocity of 10^{-2} cm sec^{-1} (the approximate average drift velocity of the charge carriers in a metal wire) then they would both be currents of 1 ampere relative to the observer (cf. fig. 2*b*).

However, they would not attract each other with a force of 2×10^{-7} newtons per metre but repel each other with a force of $1{\cdot}8 \times 10^{18}$ newtons per metre (i.e. $1{\cdot}8 \times 10^{22}$ dynes per metre or $\sim 2 \times 10^{14}$ tons wt per metre). There is therefore no need to emphasize how important the word *conductor* is in the definition of the ampere.

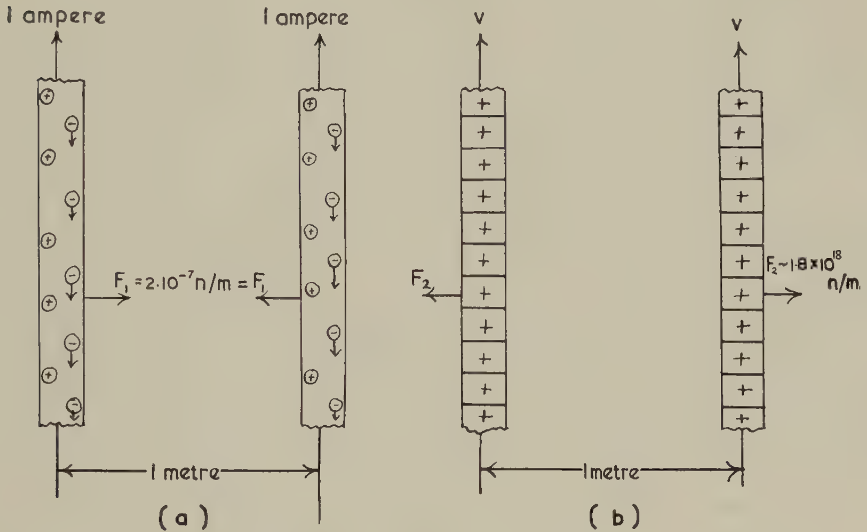

Fig. 2. (*a*) Model of a conductor in which the negative charges are moving and the positive charges remain at rest relative to the observer. (*b*) Model of two convection currents of 1 ampere.

4. APPLICATION OF MAXWELL'S EQUATIONS AND THE SPECIAL THEORY OF RELATIVITY

The discussion already presented is adequate for VIth form students, but for the benefit of teachers and other advanced readers it will now be shown that the simple formulae given by eqns. (2.1)–(2.4) are consistent with both Maxwell's equations and the special theory of relativity. Maxwell's equations and the constitutive equations expressed in gaussian units are

$$\begin{aligned}
\operatorname{curl} \mathbf{E} &= -\frac{1}{c}\frac{\partial \mathbf{B}}{\partial t},\\
\operatorname{curl} \mathbf{H} &= \frac{1}{c}\frac{\partial \mathbf{D}}{\partial t} + \frac{4\pi \mathbf{j}}{c},\\
\operatorname{div} \mathbf{D} &= 4\pi\rho,\\
\operatorname{div} \mathbf{B} &= 0,\\
\mathbf{D} &= k\mathbf{E},\\
\mathbf{B} &= \mu\mathbf{H}.
\end{aligned}$$

Here $\mathbf{E}$, $\mathbf{D}$, ρ and $\mathbf{j}$ are in e.s.u. and $\mathbf{B}$ and $\mathbf{H}$ in e.m.u. *In vacuo* $k = 1$ and $\mu = 1$. The current density vector $\mathbf{j}$ includes both the conduction and convection currents; we have omitted the term in curl $\mathbf{P} \times \mathbf{v}$ as we are assuming that the polarization $\mathbf{P}$ is zero. In our case, after the wires have reached a state of uniform velocity

$\partial \mathbf{D}/\partial t$ and $\partial \mathbf{B}/\partial t$ are both zero. Hence Maxwell's equations reduce to $\operatorname{div} \mathbf{E}=4\pi\rho$ and $\operatorname{curl} \mathbf{E}=0$, which are the conditions for the applicability of Gauss's law; and $\operatorname{curl} \mathbf{H}=4\pi \mathbf{j}/c$ and $\operatorname{div} \mathbf{B}=0$, which are the conditions for the applicability of Ampère's circuital theorem neglecting displacement 'currents'. The Hall effect, which is of the order v^3/c^3, is not present in non-conducting wires. Care must be exercised in estimating λ. We must interpret λ as the charge per unit length measured in the same system as we measure **E** and **B**, that is in a system Σ in which the charges giving rise to the convection current move with velocity v relative to the observer O at the origin. It must be emphasized that, due to the relativistic length contraction, this is not the same as the charge per unit length (denoted by λ') measured in the system Σ' where the charges are at rest relative to the observer O′. This is illustrated in in figs. 3 and 4. At this point it is well to remember that originally Maxwell's equations were assumed to hold only in the frame in which the ether is at rest; the special theory of relativity assumes that they are equally valid in *every* inertial frame.

In fig. 3 we consider the inertial frame Σ' in which the charge distributions are at rest. We will consider the charge distribution λ' e.s.u./cm made up of n' discrete charge per centimetre and each of magnitude q e.s.u. of charge. In the system Σ' the charges are at rest and the electrostatic field at the wire Q due to the wire P is $2\lambda'/r$. Hence the force on one of the charges q is given by

$$F_y'=\frac{2\lambda'\,.\,q}{r}. \tag{4.1}$$

Now consider the inertial frame Σ moving to the left along the X axis with a velocity v relative to Σ'. In the frame Σ the wires are moving in the positive X direction with velocity v. The transformation equations for forces deduced on the basis of relativistic mechanics (cf. Dingle 1940) are

$$\left.\begin{aligned} F_x&=F_x',\\ F_y&=\sqrt{(1-v^2/c^2)}F_y',\\ F_z&=\sqrt{(1-v^2/c^2)}F_z'. \end{aligned}\right\} \tag{4.2}$$

A derivation of these equations will be given in the Appendix to Paper II. These equations hold for particles at rest in the frame Σ'. Substituting for F_x', F_y' and F_z' from eqns. (4.1) into eqns. (4.2) we find that the force on q measured in the frame Σ is given by

$$\left.\begin{aligned} F_x&=F_z=0,\\ F_y&=\sqrt{(1-v^2/c^2)}\frac{2\lambda'}{r}q. \end{aligned}\right\} \tag{4.3}$$

The value of the charge q is assumed to be invariant under Lorentz transformation. We have omitted the transformation of time from Σ' to Σ:

$$t=\frac{t'+vx'/c^2}{\sqrt{(1-v^2/c^2)}}.$$

If the forces are taken to be simultaneous for all x' in Σ' they are not simultaneous for all x in Σ; but we are assuming steady conditions independent of time. The length r is unchanged under Lorentz transformation as it is measured in the Y direction.

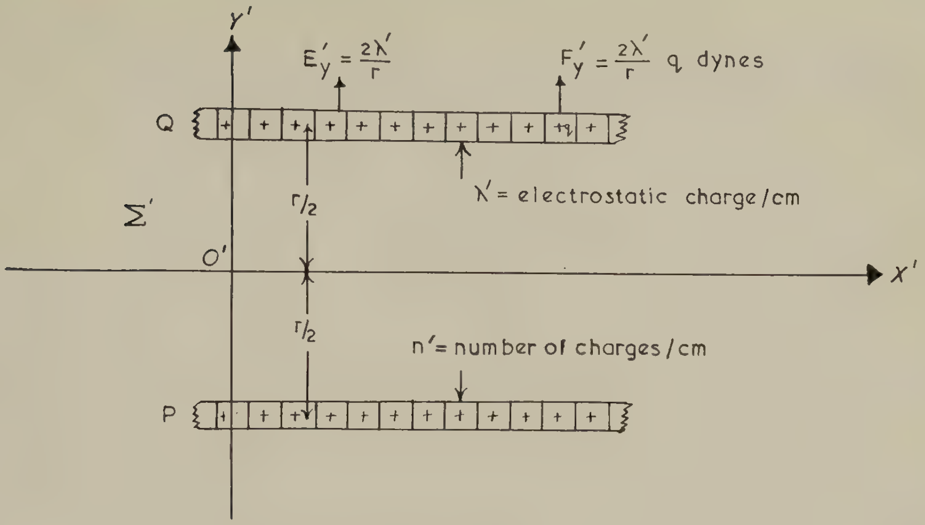
Y'
$E'_y = \frac{2\lambda'}{r}$
$F'_y = \frac{2\lambda'}{r}$ q dynes
Q
+q
λ' = electrostatic charge/cm
Σ'
r/2
O'
X'
r/2
n' = number of charges/cm
P

Fig. 3. The charge distributions are at rest in Σ'.

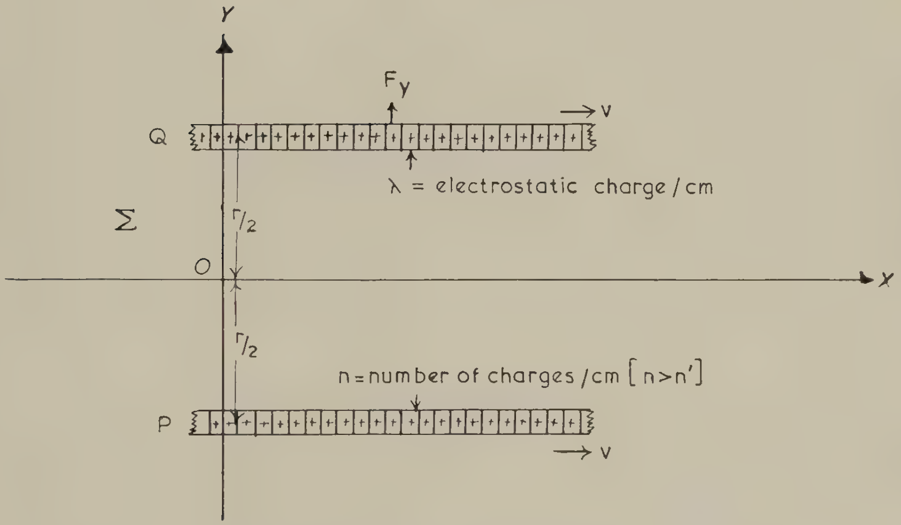

Fig. 4. The inertial frame Σ moves with a velocity $-v$ relative to Σ'. In Σ the charges move with velocity v.

Since the wires are moving in the frame Σ a length l_0 measured at rest in Σ' is reduced to $l_0\sqrt{(1-v^2/c^2)}$ when measured in Σ. Hence the number of charges per unit length measured in Σ is greater than in Σ' as illustrated in figs. 3 and 4, and is given by the equation

$$n = \frac{n'}{\sqrt{(1-v^2/c^2)}}. \tag{4.4}$$

Hence

$$\lambda = n \, . \, q = \frac{n'}{\sqrt{(1-v^2/c^2)}} \cdot q = \frac{\lambda'}{\sqrt{(1-v^2/c^2)}}. \tag{4.5}$$

Hence the force per centimetre on Q measured in Σ is equal to the product (number of charges per centimetre length, measured in Σ, times the force on each charge) and so is equal to

$$n \,.\, F_y = n \,.\, \sqrt{(1-v^2/c^2)} \,.\, \frac{2\lambda'}{r} \,.\, q = \frac{2\lambda' \,.\, \lambda}{r} \,.\, \sqrt{(1-v^2/c^2)}$$

since $n \,.\, q = \lambda$. Substituting for λ' from eqn. (4.5) we find that the force per centimetre length of Q measured in Σ equals

$$\frac{2\lambda^2}{r}(1-v^2/c^2).$$

This agrees with eqn. (2.4).

This example illustrates how the magnetic forces produced by currents can be calculated from Coulomb's law for electrostatic charges if the transformation formulae for forces given by the special theory of relativity are taken as axiomatic. The magnetic forces can be interpreted as relativity effects and represent those parts of the transformed forces which depend on the velocity of the charges relative to the observer.

5. FORCE BETWEEN MOVING POINT CHARGES

We will now discuss the example quoted in § 1. The forces between the charges can be calculated in terms of the electric and magnetic fields produced by the charges. This was done by Cullwick (1949). We will adopt the alternative approach of using the transformations of the theory of special relativity. Consider the frame Σ' in which the 'point' charges denoted by R and S are at rest as shown in fig. 5 *a*. In the frame Σ' the charges repel one another with a force $F_y' = q^2/r^2$ dynes, and the charges will start to move apart, the force on R being in the positive Y direction. We are assuming that r is very much greater than the range of nuclear forces.

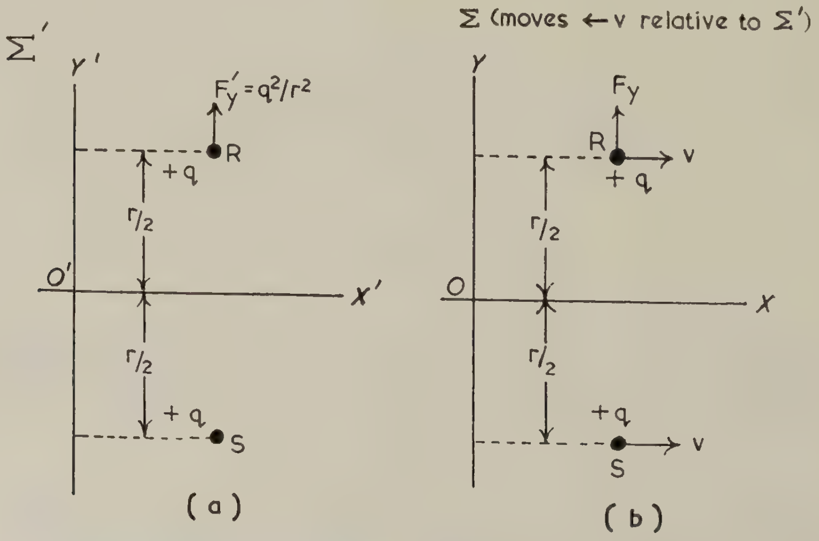

Fig. 5. (*a*) The inertial frame Σ moves along the negative OX axis with a velocity v relative to Σ'. (*b*) The charges R and S are at rest in Σ' and move with uniform velocity v in Σ.

Now, consider an inertial frame Σ moving along the negative OX axis with velocity v as shown in fig. 5 *b*. In Σ the charges move in the positive OX direction with velocity v. According to the transformation formulae for the forces on particles at rest in Σ' we have

$$F_x = F_z = 0,$$

$$F_y = F_y' \cdot \sqrt{(1-v^2/c^2)} = \frac{q^2}{r^2}\sqrt{(1-v^2/c^2)}.$$

This is the same as the formula given by Cullwick. Thus if we accept the transformation formulae of the theory of special relativity and the limitation $v < c$ then the force on the charge R is always in the positive Y direction and its direction cannot be reversed by increasing the velocity of the observer along the X axis, and the two charges should repel in all such inertial frames.

6. DISCUSSION

We have illustrated in §§ 4 and 5 how a standard result in electromagnetic theory may be calculated from Coulomb's law if the force transformations of the theory of relativity and the invariance of electric charge, are taken as axiomatic. In Paper II the general case of moving charges will be discussed from the same viewpoint leading up to a discussion of the Biot–Savart law.

As the observed magnetic forces are only $\sim v^2/c^2$ times the electric forces, where v is the velocity of the charges relative to the observer, it will be realized that quantitative magnetic experiments using charges in motion (e.g. Rowland's experiment) are extremely difficult and beyond school practical work. At school, quantitative electromagnetism must therefore necessarily be developed independently from electrostatics. The proper quantitative synthesis of electrostatics and electromagnetism can only take place after the framework of Maxwell's equations has been developed using continuous currents. In this paper it is only suggested that the relative orders of magnitude of electric and magnetic forces should be emphasized as indicated in §§ 1, 2 and 3.

ACKNOWLEDGMENTS

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Physical science for teachers in training

by F. LESTER
Hatfield Technical College

SUMMARY

An account of cooperation between Balls Park Teachers' Training College (for women), Hertford, and the Department of Science of Hatfield Technical College. Specially selected students from the Training College attend the Technical College on one day a week for a course in the Physical Sciences. The object of the course is to increase the scientific background of the students and possibly to encourage them to undertake some teaching of science in secondary modern schools.

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1. ORIGIN AND PURPOSE OF THE COURSE

A Ministry of Education (Teachers' Branch) circular of April 1956 to Area Training Organizations was concerned with ' The Balance of the Training College Curriculum '. The general tone of the circular was to point out that it was incumbent upon the Training Colleges to adjust their output, and, as far as possible, their intake, to meet the needs of the schools. To this end it may be necessary to curtail somewhat the freedom of the student in the choice of main subjects. " There are applicants for training who have good all-round educational attainments but who are disposed not to pursue in college those subjects in which the schools particularly need teachers." The gentle coercion suggested could, of course, only be applied where the College offered the necessary range of alternatives, which many do not. The greatest inadequacies in the staffing and equipping of training colleges are in mathematics and the physical sciences, where, consequently and urgently, the direct needs of the schools whose teachers are mainly supplied by the training colleges lie.

In the case of the sciences the circular pointed out that it would be difficult to induce students who came to college without prior intention of specializing in these subjects to switch to them unless there were enthusiasts on the staff. There was therefore ' something of a vicious circle ', the shortage of science teachers leading to a serious weakness in standards attained by training college students; this in its turn affecting the supply of trained teachers for primary and secondary schools.

" It was clear to the Council (i.e. the National Advisory Council on the Training and Supply of Teachers) that there is a serious shortage of teachers of mathematics and science. It was this shortage that gave most concern and should be tackled without delay."

The two colleges are only eight miles apart; there are neither laboratories nor staff for Physical Sciences at the Training College whereas the Department of Science at Hatfield has a highly qualified teaching staff and well-equipped laboratories. Thus circular and circumstances prompted the Principal of the Training College to approach the Technical College with a view to arranging a course in the Physical Sciences for a group of students and after the preliminary discussions had been completed, it was agreed that the course should be offered, and suitable students enrolled. The students were admitted in September 1957 and started their course at Hatfield, in the week after Whitsun 1958. They have just completed their course (Whitsun 1959) and have now left the Training College; the second group started immediately after the first had finished. We have thus completed one course on the original proposed syllabuses, and many alterations both in content, extent, standard, and method have been seen to be necessary. Some are being tried with the present group of students, and others will be tried in subsequent years.

The Training College is in the Cambridge Area Training Organization and the proposed syllabuses were therefore considered by the Board of Studies in Physical Sciences of the Cambridge Institute of Education. The staff of the Technical College enjoyed a free hand in drawing up these schemes of work and they were very enthusiastic about the opportunity to teach what they themselves thought would be of value to those about to assist in the teaching of elementary science in the schools.

The work to be covered was to fit into the already existing scheme of the two-year course at the Training College, and could best be done by treating the students on a ' day-release ' basis at the Technical College. The students attended on twenty-eight days, taking three hours of chemistry and three of physics between 9.30 a.m. and 5.15 p.m. In drawing up the syllabuses the main aim was to offer subject-matter covering the modern aspects of the studies; to deal with topics which were most likely to arouse interest and enthusiasm in the students; and provide a stimulus to their further reading. They were to be instructed in such topics as are currently of interest to children in the age range 10–13 so that they could have at least a basis for setting out on project work and be capable either of giving an intelligent answer to many of the children's questions, or of knowing where the answer might be found. Moreover, the intention was not to produce ' science teachers '; it was to produce ' science conscious ' teachers with an attitude to their work coloured by contact with the fascinating theories and applications of contemporary physics and chemistry. This was as much as could be done in the twenty-eight sessions at our disposal. At the outset, therefore, we were putting on a ' Special Interest ' course, as distinct from a ' Special Subject ' course. The general texture of the course was also affected by the students having completed, as part of the very interesting Contemporary Studies Course (Wingate 1956) at the college, a fortnight's study of ' Science and Society '. The fortnight's study is opened with a lecture, by a visitor, on ' The Scientific Approach '. (The students of whom I am writing were also lectured, during the fortnight, by Sir George Thomson, F.R.S.) No instruction was offered by the Technical College in the methods of teaching the subject but this was of course borne in mind by those giving the instruction. It was considered more important that the students should witness the scientific approach in operation; be shown the value of sound arguments and good techniques; and above all have transmitted to them the intense interest in his subject which the scientist has, and the scientific method as the potent way of learning that it undoubtedly is.

2. SYLLABUSES

The following draft syllabuses were submitted to the Board of Studies in 1957, and it was very pleasing to see how closely they were in accord with recommendations and suggestions circulated by the Ministry of Education to Area Training Organizations and general colleges in October 1958, on ' Non-Specialist Science Courses '. (Ministry's Reference R476(24)75. T.C. Letter No. 12 of 13 October, 1958.) It will be seen that there is no semblance of an attempt to give a rigorous course of classical physics or of chemistry such as could be found in any number of text-books for G.C.E. ' A ' level in these subjects.

Physics

General

Nature of matter. Historical approach, to modern atomic theory and nuclear physics.

Introduction, on the basis of atomic theory, to various properties of matter (magnetic, electrical, mechanical, heat, light, sound, etc.).

Force, work and energy. Energy in the electronic structure of atoms and its release in chemical action. Energy in the nucleus, and its liberation.

Radioactivity. Fission. Nuclear Reactors.

Various forms of energy—advantages of liquid and gaseous fuels, and electricity.

Prime movers to convert energy store into useful work, e.g. turbine, electric motor, internal combustion engine, jet engine.

A study of light based on atomic theory. Production of light from atoms (gaseous), Bohr's theory. Appearance of gas spectra. Spectrum analysis. Physical optics. Interference of light and thin film interference.

Cosmology

Historical development. Measurement of distances (Earth–Sun, etc.). Newton's Laws. Solar system. Stars and galaxies. Limit of observable universe. Theories of creation. Eddington's Theory. Continuous creation theory.

Life history of a star (sun). Production of energy in the sun. Effect of sun's velocity through interstellar gas.

Practical work to consist of selected experiments which will aim to develop

(*a*) insight into the course work,
(*b*) scientific approach,
(*c*) familiarity with and confidence in the use of apparatus.

Chemistry

A brief introductory survey of the scope and history of chemistry.

The development of chemistry as a science and its effect on human progress.

The concepts of atoms and molecules.

The formation of chemical bonds.

The inter-relationship between the types of bonds formed and the shape and properties of chemical compounds.

Organic chemistry—a brief history of its development followed by a study of the problems it presents and the ways in which they are solved.

The design and preparation of synthetic organic chemicals, e.g. synthetic rubber, nylon, synthetic silk, drugs and dyestuffs. Some aspects of the chemistry of carbohydrates, proteins, fats, vitamins and hormones.

Discussion of some important chemical processes: e.g. petroleum industry, gas industry, steel industry, brewing, refining of precious metals.

The chemistry of photography. Monochrome and some introduction to colour process.

Throughout the course, emphasis will be placed on scientific method.

Practical work will be organized, as far as possible, around the lecture syllabus and will aim at illustrating the fundamental principles as well as demonstrating modern methods and giving confidence in the use of apparatus.

Such a course can only be embarked upon when the lecturers concerned not only know their subject well, but are prepared to experiment with new methods of presenting it to a class whose previous history and future requirements are not only heterogeneous, but completely new to them as Technical College teachers. The intention was not, of course, to cover all the topics mentioned but to see how the work developed, and to use the parts of the syllabus best suited to the circumstances. The main considerations would naturally be the particular interests of the lecturer, which would affect his approach to the subject when given comparative freedom, and the previous history of the students.

In the physics course there was naturally great interest in atomic structure, isotopes, fission, fusion, and all the related phenomena and practical applications. This was exploited to the full, and the students were given a considerable amount of reading to do. Furthermore, ' Sputniks ' had just hit the headlines of the daily press, and these also became a centre of interest as well as serving to ease the way to an understanding of the Bohr theory. The analogy between electrostatic attraction and gravitational attraction seemed readily acceptable. Because of the various standards previously reached by the students (some had taken physics to ' O ' level and some to ' A ' level of the G.C.E.) intricate experimental layouts had to be avoided. The experimental results must have an intrinsic interest and the demonstrations should be very much more than something to look at. The students should themselves be able, afterwards, to set up the same demonstration with confidence.

Just a few of the demonstrations and pieces of experimental work may be mentioned as may also the fact that many films from various sources were used throughout the course. Typical experiments were: (*a*) the determination of interionic distance, using rock salt; (*b*) the discrete nature of radioactive decays and the relative penetrating power of the various radiations (the students examined, under a low-power microscope, the individual ' atomic explosions ' occurring in luminous dials of wrist watches); (*c*) the measurement of the wavelength of the red line in the hydrogen spectrum, using a spectrometer, a diffraction grating, and a discharge tube. This experiment was carried out after some weeks of discussion of the Bohr model of the hydrogen atom, and the emission or absorption of energy on transfer of electrons from one orbit to another. The good agreement between their experimentally obtained value for the red line and that predicted theoretically made a considerable impression on the students.

The cosmology was presented with the aid of several films, of which the best were some Russian colour films with English dialogue. One of the students organized a project on ' astronomy ' which lasted through the period of her junior school practice (age group 9–10), was extremely well done, and brought forth an astonishing volume of press cuttings, pictures, and contributions from the children.

The lecturer responsible for the chemistry teaching found a most receptive audience for his work involving the application of the studies of atomic structure already covered in physics, and developed this to show the ' shapes ' of orbitals and how bonding occurs. Naturally, there was great interest in the modern synthetic fibres, drugs, and dyestuffs, but there was insufficient time to touch on all the topics in the syllabus. The section on chemical industries was omitted completely; it may be mentioned in passing that the lecturer was an organic chemist with an interest in photography ! It was rather difficult to organize suitable laboratory classes in this subject at the beginning of the course because of the introductory nature of the talks. We therefore decided to show some modern analytical techniques, the value of which would be understood when the complex nature of amino-acids, for example, was appreciated. The students were accordingly given a course of chromatography as a means of identifying, separating, and purifying complicated mixtures of similar substances. Each one hydrolyzed some of her own hair, and used paper chromatography to show the different amino-acids thus formed; they also used a column of alumina to show that grass extract contained more than chlorophyll. Substances of topical interest were prepared, among them aspirin, sulphanilamide, and methyl orange as examples of drugs and dyestuffs; the latter was used to dye a piece of material. The optical properties of substances were investigated, and the students used the polarimeter and spectrophotometers.

Towards the end of the course we made two visits. The first was to the Department of Aeronautical and Mechanical Design at the College where amongst other things the students saw the wind-tunnel, and a demonstration of the shock wave produced at an aerofoil section in the supersonic wind-tunnel. The second was to U.K.A.E.R.E., Harwell, where they saw some of the work in the ' hot labs ', BEPO, and ZETA which was being dismantled at the time.

A great deal of detail could be written about the particular choice of subjects and experiments, and the measure of success achieved in each case and with the course as a whole. What can be done in a few words, however, is to summarize the ingredients necessary for success; those which we were fortunate enough to have in full measure for the first year of our cooperative venture.

The girls were carefully selected by the Training College; all had studied at least one of the subjects up to the standard of G.C.E. ' A ' level, and some had studied both. They were full of enthusiasm, and prepared to work hard, knowing full well that they were ' guinea-pigs ' and that the continuance of the course depended largely on them. The lecturers were young men with the energy and ability to prepare, arrange and integrate their work carefully. Equally important was the close cooperation between the two Colleges and the Cambridge Institution of Education.

Of the six girls who attended the course, all have accepted appointments as science teachers in secondary modern schools; it would appear that our ' Special Interest ' course has given them sufficient to make them want to pass on their knowledge. There are six more teachers of science than there would otherwise have been. Nine girls are attending the course this session. It may safely be said that the girls have derived considerable benefit from their course at Hatfield; many a student here would be ready to say that the converse is equally true.

ACKNOWLEDGMENTS

I would like to record, with appreciation, the efforts of Miss G. Furniss of Balls Park Training College, who supervised and organized the study periods allocated to the course and kept up an efficient general liaison, as well as those of the lecturers, D. J. Finnett (Physics) and Dr. G. W. Wood and Dr. G. G. Dearing (Chemistry).

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WINGATE, M. M., 1956, *National Froebel Foundation Bulletin* No. 100, p. 2.
PLATO FILMS LTD., e.g. ' *The Great Universe* ' ' *Eclipse of the Sun* '.

Churchill College

by Sir JOHN COCKCROFT

Master of Churchill College

On 24 January 1959 the Council of the Senate of the University of Cambridge approved the foundation of a new college to be called Churchill College, as a memorial to Sir Winston Churchill.

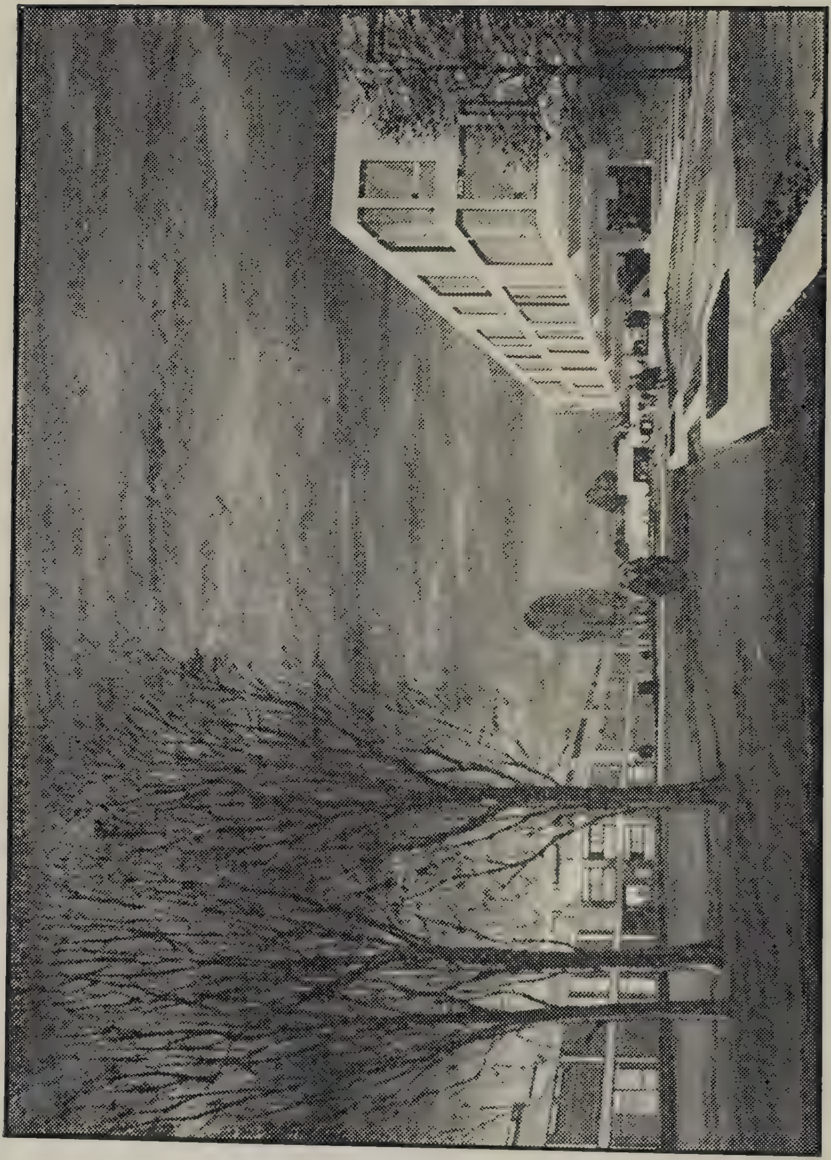
The foundation of the College has been made possible mainly by the generous response of British Industry to an appeal by the Churchill College Trustees for the subscription of £3½ million to found the College. Up to the present time £3·4 million has been subscribed and over 65 per cent of this has come from industry. The remainder has been subscribed by Charitable Foundations, Banks, Insurance Companies, City Companies and individuals.

The objectives of the Trustees in founding the new College are first to increase the output of the high quality scientists and engineers trained by the University of Cambridge. Second, to promote and encourage post-graduate courses of study, especially for men with some experience in industry, and third, to attract post-graduate students not only from this country but from the great English speaking world overseas.

To promote the last objective a United States Churchill Foundation has been established and it is hoped that funds may be raised to provide for Visiting Professors and Churchill Students and Scholars who may enrich the life of Cambridge in the future as the Rhodes Scholars have enriched the life of Oxford.

During the last few years the University of Cambridge has greatly expanded its teaching and research accommodation in Engineering, Chemistry and Chemical Engineering and is making provision for an expansion in Biochemistry whilst an expansion of accommodation for teaching in Physics and Metallurgy is being planned. This expansion has not been accompanied by a similar expansion in residential accommodation so that most of the existing colleges are overcrowded and a great deal of double banking on accommodation has been necessary. Some expansion of residential accommodation in existing colleges is proceeding but this will be inadequate to provide all that is necessary. Only one new men's college has been founded in Cambridge during the last century, a remarkable contrast to the 16th century when six new colleges were founded. The tremendous contribution made by these colleges to the life of the nation is illustrated by the single case of Trinity College which has been the home of Newton, Clerk Maxwell, Rayleigh, J. J. Thomson and Rutherford and innumerable other scientists of distinction. If Churchill College can contribute in a similar way in the years ahead its foundation will have been many times justified.

In order to make an important contribution to providing accommodation for more scientists and engineers the Trustees have decided that the College should ultimately have 360 undergraduates, 180 advanced students and 60 Fellows. About 70 per cent of these would work in the fields of science and engineering and 30 per cent would be art students. This proportion of arts and students should be sufficiently high to promote that mixing up of students of all disciplines which is the best form of general education at the university stage.



Design for Churchill College: the Great Court

The Trustees also hope to provide accommodation in College for the great part of the unmarried students, since lodging accommodation is becoming increasingly difficult to obtain and many members of existing colleges have to live several miles away from their college. The Trustees are also going to build 20 small flats for the use of married visiting professors, advanced students, and others coming to Cambridge for limited terms.

On the advice of the consulting architect, Sir Leslie Martin, the Trustees organized an architectural competition under R.I.B.A. rules. Twenty nominated architects competed, amongst them being many of the best young architects in the country. From the first round, four firms of architects were selected by the assessors to prepare more detailed drawings and from these a final selection was made, and Messrs. Sheppard, Robson and Partners were appointed.

The winning design is shown in the perspective drawing. It provides for 525 residential quarters arranged mainly round a court about the size of Trinity College Great Court. Surrounding the Great Court are smaller courts with arcades connecting them on the ground floor so that it will be possible to walk from one to the other under shelter. The small courts will accommodate 40–50 students and Fellows in buildings of two or three storeys. Staircases at each corner will lead to groups of four or more rooms on each staircase and each landing will have common services. About two-thirds of the accommodation will be in the form of bed-sitting rooms and the remainder will be two room sets for junior members of the College. There will also be some larger sets of rooms for Fellows living in College.

The central buildings of the College will include the Hall and Common Rooms; a lecture and assembly room to seat about 300, where lectures, concerts, plays and films can be heard and seen. This will probably prove to be a focus for innumerable summer schools of the future. There is also to be a Brendan Bracken Memorial Library devoted primarily to a large undergraduate reading room and the books which they need for their studies. There will also be an Ernest Bevin Memorial Library adjoining this for the accumulation of archives, reference books, books used for research and micro films of periodicals. The Bevin Library will include seminar rooms and a small lecture room. There will also be rooms where student societies can meet and society dinners can be held.

Residential accommodation will be provided for the Master and the Senior Tutor and they will have small private gardens.

The residential accommodation will be built mainly of brick with some stone facings and the central buildings will be faced with stone. The College will be centrally heated throughout to reduce the service staff required, and attention will be paid throughout its design to ease of service.

Beyond the Great Court will be the playing fields separated on the Madingley Road side of the College by an ornamental water. Sufficient acreage is available to provide for six pitches for winter games as well as for twelve tennis courts and four squash courts. Space is also hopefully reserved in the plan for a covered heated swimming-bath, but this will depend on whether special funds are subscribed for this. The block of flats for married members of the College will be sited near the Observatory and space will be reserved at this end of the site for houses for senior members of the College. The cost of the complete College buildings, including site preparation, playing fields, furniture, fees, etc., has been estimated at about £1·7 million. If this figure is confirmed by the actual tenders received, the remainder of the sum subscribed will provide sufficient endowment income to provide for stipends, maintenance, rates, repairs, university contributions, etc., for the hard core of activities of the College. It will not provide for some of the important features of the College, such as visiting professorships, Senior Research Fellowships, etc., which the Trustees have thought to be very necessary. Benefactions of the Ford and Rockefeller Foundations will provide for them during the first five years or so of the life of the College.

The appointment of the first nucleus of Fellows of the College was entrusted to an Appointments Committee consisting of the five ' Cambridge Trustees '. The Senior Tutor is to be Mr. John Morrison, a Classicist, at present Senior Tutor of Trinity College. He will bring with him the great experience of Trinity College and its Scholarship Group in selecting undergraduates. The Bursar is Major-General J. Hamilton, an ex-sapper and an ex-member of Caius College. Dr. P. G. Ashmore is to be Tutor for advanced students, and the Honourable Richard Adrian Tutor in natural sciences. A start has also been made in appointing College lecturers—the teaching Fellows who will be responsible for the supervision in the College.

The statutes and ordinances which define the rules for the government of the College have been drafted during the last year and will now have to be approved by the University and Privy Council.

The College will formally come into being when the first students come into residence. It will then become an 'Approved Foundation ' and its students will be able to proceed to degrees in the University. For a time the Trustees will retain control of the trust fund since the greater part of the sums subscribed will accrue under covenant over a period of years. A College council will, however, be established to be responsible for the government of the College and for the election of Fellows, students and scholars, and after a few years the College will be completely self-governing.

The undergraduate members of the College will be admitted through the well-established machinery of the scholarship examination and by a general entrance examination for commoners. For this purpose the College has joined the Trinity College group which comprises Clare, Downing, Jesus, Magdalene, Trinity Hall. The character of the general entrance examination is changing in the endeavour to discourage the excessive specialization in the schools which is deplored by most educationalists.

The College intends to strengthen the direction of studies of junior members of the College by whenever possible combining the duties of tutor and director of studies. Most of the detailed supervision of students will be carried out by the usual tutorial system, mainly by College lecturers who are Fellows of the College supplemented by assistant lecturers. The College will also provide some supervision for advanced students taking post-graduate courses of study.

It is also hoped that the visiting professors and senior research Fellows will contribute to the educational life of the College by giving general lectures and seminars. The College may also be able to contribute to University education by arranging seminars in those growing points of science which are not at present formally taught. We will be willing to experiment with our educational policy and learn as we go along, as befits the most junior of all the colleges of Cambridge.

A clearing house for physics students

by L. R. B. ELTON
Battersea College of Technology

By Christmas last year one London college had had 900 applications for its Honours Physics school for the following October; another had 500. The total number of students admitted to Honours Physics schools throughout the country in 1957 was 1314† and the number of places available was probably around 1400. Now of those that apply so early in the year about 30 per cent will fail to obtain the necessary entrance qualifications, so at a liberal estimate the total number of applicants throughout Britain should not exceed 2000. If it does, then worth-while students will be turned away. If it does not, then the numbers quoted at the beginning of this article are merely a reflection of the quite unnecessary panic in which sixth formers and their teachers find themselves at present. Certainly, in my experience, few intending students apply to less than five colleges nowadays and many apply to more.

With all this in mind I decided to find out the facts, and so during the summer I made it known through the press that I wanted intending students who had been unable to find a place to get in touch with me after 31 August. I also expressed the hope that I might be able to place some of them. I then circularized all universities and technical colleges that offered courses either for the B.Sc. Special in physics or for the Diploma in Technology in applied physics and suggested that they might let me know of vacancies, if they were willing to cooperate in such a clearing-house scheme. At this stage I felt that it was too complicated to cater also for the General Degree.

Perhaps I should say at this point what a clearing house is not. It is not an office which distributes students to Universities, thereby restricting the freedom of students and usurping the position of admission tutors. Its purpose is simply to help those students that have been unable to find places unaided, and to fill the vacancies at Colleges that normal methods of student application have failed to fill. My task therefore was merely to inform an intending student of a vacancy which was in keeping with his qualifications and for which he would then apply himself. The final decision rested—as it must—with the admission tutors. I will admit that I had a theory that students who turned to me for help would not be too particular as to where I could place them. Although this was generally true, there were exceptions.

My coverage of universities and colleges was complete, while that of intending students was not, since it depended on individual students hearing about my letter in the press. Unfortunately, the newspaper which might have been most useful in getting my offer widely known felt itself unable to publish it because of lack of space. Also I hope that no student was put off me by the quite wrong report that I had referred to him as ' flotsam and jetsam '. If that was my opinion of students, I would choose a different profession.

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† N. Clarke and J. Bowes, A Second Survey of Available Places for Physics Students in Universities and Technical Colleges, *Bull. Inst. Phys.*, **9**, 143 (1958).

Because of the difference in coverage it is not possible to draw entirely reliable conclusions. I received replies from about thirty universities and colleges. Most of them were negative, but that was not unexpected, particularly since, being of an egalitarian nature, I had circularized even Oxford and Cambridge. On the other hand, I did have offers of about 50 places for the B.Sc. Special Degree from seven different universities and of about 50 places for the Diploma in Technology from eight colleges of technology. Special mention must be made of one university that offered to increase its intake by five students so as to help my scheme. Alas, the entrance requirements were so stiff that I could not even send one. I also had about 30 places offered to me for the London External degree, but there seems to be a very small demand for this.

The number of students who wrote to me was 28. To these I sent a questionnaire which was returned by 19. Of these I placed three in degree courses and one in a full-time course for the Diploma in Technology. A further five had offers of places independently while they were waiting to hear from me, two decided to stay at school for a further year, one took a job, and one could not be placed because he lacked A-level mathematics. Six have not had the courtesy to let me know whether I have succeeded in placing them or not.

The most obvious conclusion that can be drawn is that no good student should have failed to get a place in an Honours school. None of those that applied to me had brilliant qualifications; they mostly had three A-levels with an average mark varying from 40 to 58. Now there are colleges that insist on a minimum of 65, but in my experience many a one with 45 has got a respectable degree, largely, I should think, because the G.C.E. is a somewhat fallible prognosticator. So, while there was therefore no great need this year for a clearing house, the margin is uncomfortably small, the number of vacancies being of the order of 3 per cent. This then is the explanation of the present rat race of applications combined with the obvious absence of frustrated Rutherfords among the National Service men of the last few years.

The situation is somewhat different for the Diploma courses. Here the number of vacancies is running at about 30 per cent and it is undoubtedly true that some intending students who fail to obtain a place for a degree stay a further year at school rather than study for the Diploma. I do not wish to touch here upon the problem of ‘ Degree or Diploma ’, but it seems to me that teachers in schools should give very serious consideration to the problem of ‘Another year at school or Diploma ’.

There were three girls among the applicants, and although this number does not enable one to draw statistical conclusions, it is worth noting that in order of examination marks they came first, sixth and seventh among the nineteen, and that I placed two of them. It must also be recorded that as letter writers they far exceeded their male colleagues. They all replied and they could all write decent English. An extract from one deserves to be quoted, as in itself it is a justification of the scheme: “ I do not know how I can thank you for all you have done and in the next few years I shall try to justify all your hard work.”

Fortunately there was not much hard work in it. I reckon that I spent a total of two days and my secretary a total of one day on the scheme and that if these times are turned into cash and the cost of stationery and postage is added, the total comes to under £10. At £2 10*s*. 0*d*. per placement this seems cheap. I understand that to attract a graduate into an industrial firm may cost anything up to £1000.

It is doubtful whether my conclusions will stop intending students from applying to half a dozen places simultaneously. For most students there is more to it than finding a place somewhere. They are certain that there is a definite hierarchy which begins with Oxford and Cambridge and descends via Redbrick to Whitetile. Failure rates of 25–30 per cent at some universities may indicate that the 'best places' are not necessarily the best places for everyone, just as many a secondary modern schoolboy will succeed in G.C.E. in a way he might never have done had he been at a grammar school. Some of the letters I have had have certainly given me a glimpse of the prejudices not only of the students, but also of their teachers.

I intend to repeat the scheme next year. At present I am clearly dealing with what might be described as frictional unemployment. But the day may come, when the bulge enters the universities or perhaps when physics gains more adherents, on which the present delicate balance is upset and I am swamped with applicants. On the other hand, a clearing house may act as a barometer which will give sufficient warning of such a situation for remedial measures to be taken in time. So if any intending student is unplaced on or after 31 August 1960 would he please write to me at Battersea College of Technology, London, S.W.11, enclosing nothing except a stamped addressed envelope, size 9 in. by 4 in.

Foundations

A SYMPOSIUM OF VIEWS ON THE I.A.P.S. REPORT

The booklet 'Foundations; A Reconsideration of the Aims of Teaching in Preparatory Schools', the Report (published in 1959) of a Committee appointed by the Council of the Incorporated Association of Preparatory Schools, has an interest far beyond the relatively narrow circle of these institutions, particularly in the sections concerned with the teaching of science. For they, of all primary schools, have had by far the longest experience of equipping their pupils directly for a further school career; and they have, on the whole, given little attention until recently to the place of science in their curricula. It is therefore to be expected that, approaching the problems of teaching introductory science to the very young with fresh and open minds, they may have much that is valuable to offer to the wider educational world.

The Report recommends the introduction of some science, formal or informal; and gives specimen syllabuses, with the following observations:

(i) *Astronomy* holds great appeal and can be started young and continued by plotting the stars and planets and the position and shape of the moon, or by visits to a planetarium.

(ii) *Measurement* meaning the standards of length, volume and weight, is the right subject with which to start off real physics. Here is the proper application of elementary mathematics and a vital part of observation.

(iii) *Physics* must be watched carefully lest too much be attempted and the surface only skimmed. The emphasis should be on the application rather than on the principles, which are usually difficult for a young mind to grasp. There should be plenty of demonstration and practical work.

(iv) *Elementary Chemistry*, if taught, does not require elaborate apparatus for even if the experiments are confined to those requiring only test-tubes, beakers and dishes, a great deal can be learnt.

(v) *Weather Charts*, *Rain Gauges*, and various kinds of Nature Study offer practical opportunity for leisure activities.

(vi) *Biology* and *Physiology* are useful additions.

(vii) Every school Library should have a Science section including simplified biographies of the great scientists.

We invited the views of a preparatory school headmaster, Mr. H. Clutton-Brock (Cheltenham College Junior School); a senior science master, Dr. J. G. Goodier (Eton College); a public school headmaster, Dr. B. M. W. Trapnell (Denstone College), who mentions the recent discussion of the Report by the Headmasters' Conference; and the Chairman of the Education Group of the Institute of Physics, Mr. G. W. Warren. It is hoped that these may stimulate further comments and correspondence.

Mr. H. Clutton-Brock writes:

The authors of this official I.A.P.S. booklet have wisely, and successfully, set out to suggest the ideal curriculum for preparatory school boys, regardless of the limiting effect of present exams, and it deserves careful study by all interested in education, and independent education in particular.

The prep. school boy has to cope with a full curriculum ; leaving his kindergarten at eight, having done little more than make a start on the three Rs, he has, some sixteen terms later, to face a public examination in nine subjects. No wonder there is sometimes criticism that he has been taught everything and knows nothing! Undoubtedly there is need for a curriculum which will lead to less exam-cramming and enable more English, Music, Art and Natural History to be taught ; but this can only be achieved by making room for these subjects, and the suggestion is made that Latin—that subject of much present controversy—should be started later and not all boys should have to continue it throughout their prep. school careers. This surely is a major step in the right direction, for most prep.—and I believe public—school masters would agree that for many boys much time now spent on Latin could be better employed.

The other major suggestion is that Science should be taught in every prep. school, Natural History leading on to such branches as Astronomy, Chemistry and simple Physics. There are strong arguments for this: that it stimulates a boy's interest at an age when he wants to know ' how it works ', that it encourages observation and accuracy, that it gives him an introduction before he reaches his public school, where he may immediately have to make an irrevocable decision whether Science shall be his chief option. On the other hand, will prep. school science really make a contribution or shall we, by experiments with old tin cans, bits of meccano and torch batteries, give a boy a false and facile picture of what Science really entails ? Do public school scientists wish boys coming to them to know something of the subject or do they prefer to start ' ab ovo ' ? Might it not be possible for public schools so to organize that a boy does not have to make a final option decision until he has been there a year, and leave to prep. schools the task of whetting his appetite by confining its activities to Natural History, encouraging a boy to use his eyes and leisure well ? And anyway, can we in prep. schools acquire staff competent to teach him well and make a worth-while beginning which will be acceptable to his public school ?

These are questions that need answering before Science takes its place as an accepted prep. school subject, and it is the scientist who should be heard before a final decision is made.

Dr. J. G. Goodier writes:

The introduction of Science into the curriculum of preparatory schools is necessary, and the sooner it is brought about, the better.

Under present arrangements, a very able boy entering a public school is already nearly up to ' O ' level in Latin, but often quite ignorant of the simple scientific principles which underlie many everyday experiences. His inclination will be towards the Arts side from the outset, for it is in this area that he feels most at home, and is therefore likely to make the more rapid progress; much courage is required of him if he is to elect to change to subjects with which he has but meagre acquaintance. Even if, as is often the case, he reaches G.C.E. ' O ' level (or its internal equivalent) in Science in time, he may not be very successful, because in a hurried course he simply will not have lived with the ideas long enough. A scientific outlook cannot be brought about by an education of learning by rote.

As always in educational matters, there are conflicting requirements. The preparatory schools need as much freedom as possible to teach what in individual circumstances they can do best; but if the result is to be of much use, the public schools must have some idea about the foundations they will have to build on, and this seems to dictate the need for some sort of basic syllabus.

In general terms it looks as if the sort of science done in a good secondary modern school or in the first two years of a grammar school, will be needed. Any adult of reasonable intelligence and a lively mind could teach this, but there are techniques peculiar to science teaching about which an intending teacher should know. Courses for teachers and assistance with such matters as economical equipment of laboratories will be essential, and ways will have to be found to finance these. This work might be undertaken by some of the public schools; or perhaps school laboratory equipment firms could offer courses, as is done in Germany.

The next move, however, is for the public schools. It is more than probable that they could devise a selection procedure which could encourage earlier introduction to

Science, and improved teaching of Mathematics in the preparatory schools, and at the same time, render 'cramming' for entrance less effective. The present system is not wholly satisfactory in this respect.

The foregoing has dealt with the limited problem of Science teaching. With the Report as a whole I find myself in substantial agreement; it would obviously be a gain if preparatory schools, in addition to feeding the public schools, could send boys to grammar schools at 11 or 13, or accept them at 11+ from primary schools. The proposals seem to make this feasible.

Dr. B. M. W. Trapnell writes:

At the Oxford meeting the Headmasters' Conference discussed the question of Preparatory School Curriculum, basing their discussion on the Report entitled 'Foundations' which the Incorporated Association of Preparatory Schools recently commissioned. In this Report most sides of preparatory school curriculum are examined, a number of important questions are raised and possible new timetables considered. Altogether the Report is a very valuable document.

According to 'Foundations' the case for science teaching in preparatory schools rests on three points:

1. Science can stimulate interest in natural things and supplement a boy's interest in how things work. It can also encourage faculties of observation, measurement and recording.
2. Science should be on an equality in preparatory schools with History, Mathematics, Classics, etc., as a possibility for later specialization.
3. There should not be a compulsory science paper in Common Entrance, as this would hinder the creation of a lively interest in the subject and restrict freedom of choice of subject matter.

In addition the authors of 'Foundations' express their belief that science courses at preparatory schools need not necessarily be run by qualified scientists.

The preamble indicated general agreement with these points, and favour was expressed with the service initiated by the Esso Company whereby elementary textbooks concerned with a particular scientific topic (e.g. Water and Life) are circulated free of charge to preparatory schools, together with kits of apparatus which cope with the experimental side of the topic in question. Certainly this service, due to Mr. John Lewis of Malvern, has done and will continue to do a great deal to encourage preparatory schools to commence Science.

While many people agree with these points, a number of objections are still voiced. Two common objections are that boys may be tempted to specialize too early through Science teaching at preparatory schools, and that preparatory schools will only make a muck of a boy's early scientific education. The answer to the first point has been provided by Lord James: "Eleven periods a week of Latin from the age of eight is called a broad general education, and three periods of Science a week from the age of eleven is called being in danger of premature specialisation ". Certainly few people indeed would want more Science teaching than this in preparatory schools, while everyone would deprecate the creation of anything like a science side at preparatory schools. The answer to the second objection is surely that one must first decide whether science teaching in preparatory schools is correct, and if so, see that it is done properly. To say in effect that one would like to teach science in preparatory schools, but unfortunately . . . is a thoroughly bad principle, and anyhow a number of preparatory schools are already teaching science, not only in excellent little laboratories using excellent equipment, but also with very well qualified teachers.

For the Common Entrance boy, who will have either two or three years at his public school before sitting 'O' level the informal type of science teaching implied by the Esso service, and the absence of any Common Entrance paper in science, seems sensible. Besides stimulating a number of interests in a boy, it will familiarise him with the language and method of science, the work that he does will remove some of the present compression of the 'O' level course at public schools, and he will appreciate better some of the physical quantities he deals with in Mathematics. But the problem of the scholarship boy is a little different, and my own personal view is that it has not been fully realized. In essence the problem appears to me as follows.

Many public schools, though admittedly not all, place their better scholars in ' O ' level forms: in almost all subjects such as Mathematics, Latin, French and English, such boys are sufficiently advanced rightly to enter a certificate form. But it they do so, their science is out-of-step *unless* at their preparatory school they have done the equivalent of the first year Science course at public schools. Is it not right for these boys to have formal science teaching in their last year at preparatory schools ? If they do not their education is lopsided—advanced in most subjects, retarded in science.

The results of the present arrangement may be seen at some schools, where attempts are made to bring boys up to ' O ' level standard in one year or four terms, even though they have little or no prior knowledge of science, and up to 'A' level standard in three years. In one such school the pressure put on the boys is lamentable in the extreme: in another the headmaster at least looks a bit sheepish when questioned on the matter!

My own overall impression was that while ' Foundations ' has brought main issues into the open and caused a number of people to realize what they are, there is still some way to go before the issues are fully understood.

Mr. G. W. Warren writes:

> Preparatory schools, with their curricula largely dictated by the requirements of the Common Entrance examination, are very naturally conservative in outlook, and the Report of the I.A.P.S. reveals considerable courage in reconsidering the aims of these schools and their position in the present educational system. Many parents, faced with the prospect of heavy expenditure at a university, must choose at what stage in their boys' education money is best spent and at what stage they avail themselves of facilities offered by the State in maintained schools. The Report recognizes this problem and makes sensible recommendations to ease the transition from or to the maintained schools.
>
> The I.A.P.S. is overwhelmingly in favour of introducing some science ' formal or informal ' whatever this phrase may mean, and the objections which the Report says must be faced at the outset are not convincing. One is led to suspect that there was a strong rearguard action in the course of discussions. The approach to the teaching of science is rather timid and although the Report concedes that science as a subject should be on an equality with history, geography and classics, this observation is followed by a suggestion that expert scientists are not needed on the staff and that masters who are " sufficiently mechanically minded to enjoy this work " will be competent to teach science. Again, the statement that in physics the emphasis should be on applications rather than on the principles is surely giving the subject a wrong perspective. Applications, of which many boys are already aware, can be used to illustrate fundamental principles, but it is the latter which are important. ' How and ' Why ' are the insistent questions which young boys seek to have answered and the good teacher is one who can communicate fundamental ideas simply and accurately.
>
> The preparatory schools in this country have a fine tradition which has grown from the quality of the teaching, and it is nearly true to say that it matters little what particular curriculum is followed provided that what is taught is taught well and by teachers who can inspire.

These are considered views on the document; and it is encouraging to be able to report action along the lines it suggests.

The Esso Petroleum Company's loan scheme for kits of apparatus is based on the experiments in three books of the ' Science on the March ' series, published by Longmans. These are *The Weather and the Earth*, *Energy and Engines*, and *Hearing and Seeing*. Mr. Lewis has devised the kits, and also prepared teaching notes which will be valuable guides to the inexperienced demonstrator. At present their issue is on a pilot scale, but the Company has been inundated with requests, and it is understood that the kits will shortly be made available for purchase through the usual suppliers.

Rather more formal, and also more general, is the science syllabus suggested at the Denstone College course for preparatory school masters held there in the Spring of 1959. The elementary physics of heat and light, and of air and water, is studied experimentally at first in its own right, and then in relation to chemistry and biology. The mechanics of ' Energy and Engines ' is missing, but in its place is an appreciation of the part played by physical ideas in the related sciences. The cost of the apparatus suggested for the Denstone course is about £42. Expressed as a fraction of the price of the normal range of modern teaching aids, this is extremely small.

Two other points should be mentioned. Lewis (1959) mentions the hope that when the subject has become widely established, some examination will be devised in order to command the respect it deserves. This is a matter of policy for the future, but may well lead to some useful reconsideration of methods of examining in science. The second point is the ultimate gain to be expected. Nobody supposes that a boy reaches the age of 13 without an awareness of the applications of scientific principles in the world around him, or that he runs short at any time during the preceding decade of intelligent questions to ask about them. He is probably fuller of the spirit of scientific enquiry at the preparatory-school age than at any other time in his life: encouragement and guidance in the early years may be far more valuable than skilled tuition for university entrance and exit later on in producing the kind of scientist, and the kind of educated person, that the country needs. And this, of course, goes for all primary education.

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ESSAY REVIEWS

The Spiritual Crisis of the Scientific Age. By G. D. YARNOLD. (London: George Allen and Unwin, 1959.) [Pp. 208.] 18*s.*

Recently seven well-known scientists gave a series of addresses in Great St. Mary's at Cambridge in which they spoke about religion from their own point of view. (The addresses have been published under the title *Religion and the Scientists*, S.C.M., 5*s.*). Commenting on them the Bishop of Southwark remarked that the series did little to remove the dichotomy between science and religion—but the speakers did at least consider religion to be worthy of serious thought, however critical they might be of many doctrines of the Christian Faith.

This attitude, which I believe to be typical, represents a stage in the movement away from the bitterness of the controversy between science and religion which has harmed both for more than a hundred years. In the early years modern science looked to the Church as an ally. Clergy were among the early members of the Royal Society and bishops helped to found the British Association. As Dr. Yarnold points out in the opening chapter of his interesting book, the scientific attitude to nature and the reformation attitude to theology have much in common. But the theologians were too dogmatic in their interpretation of the Bible and the scientists in the enthusiasm of their search for truth perhaps too ruthless in their questioning of tradition. The cleavage between science and religion was already in existence before Bishop Wilberforce and Huxley had their historic quarrel at the British Association meeting. The division and hostility between science and religion has had disastrous results for which the Christian Church must take no small part of the blame, and it is only in recent years that hostility has been replaced by a kind of benevolent neutrality. Now it would seem that an increasing number of scientists realize that science cannot answer to the whole of man's experience, while theologians for their part can see in the achievements of physical science not only the guidance of the Holy Spirit, but fresh understanding of God's nature and purpose. Thus Professor Coulson can say—I believe in a truly Biblical sense—that " science is in itself an essentially religious activity ", while a Biblical scholar like Professor Dodd can write that " the best statement we can make is nothing more than a very inadequate symbol of ultimate reality ". As the Vice-Chancellor of Manchester said recently: " For the first time there is something like a genuine scientific agnosticism, an attitude of humble realisation, not that science does not provide the answers but that in the nature of things it probably never can ". And the dimension of faith enters in as when Professor Polyani wrote: " Science or scholarship can never be more than an affirmation of the things we believe in. These beliefs will by their very nature be of a formative character. Claiming universal validity they must also be responsible beliefs held in due consideration for evidence and of the fallibility of all beliefs: but eventually they are ultimate commitments, issued under the seal of our personal judgement. To all critical scruples we must at some point finally reply, ' For I believe so ' ". (*Logic of Liberty*, p. 31.)

While there is now little conflict between science and religion it would be misleading to assume that there is now basic agreement. But scientists and theologians are learning to understand and to respect each other, thanks to the work of scholars like Dr. Karl Heim and Dr. Raven, Professor Coulson and many others. And we owe much to the work of interpretation carried on by such writers as the Rev. Dr. Smethurst in his book *Modern Science and Christian Belief*, published just before his untimely death. Dr. Yarnold himself, by training a physicist, has been engaged for some years on this task of interpretation and his latest book is a valuable contribution to mutual understanding.

In it he refuses to consider the relationship between Christianity and the natural sciences as a conflict, even as a conflict to be resolved. Instead he sees the modern world as passing through a crisis of far-reaching proportions which is the direct consequence of its inability to assimilate a vast increase in human knowledge and to direct rightly a prodigious growth of technical power.

The book falls naturally into three parts: the first is concerned with the pattern of the external world leading to a Theology of Nature; the second deals with Biblical criticism, with miracles and with communication of the κερυγμα—the essential New Testament Christianity—to the modern scientific world; the third section relates Christian ethics to problems created by scientific and technological advance.

As a historian and a student of theology I am not competent to pass judgment on many parts of the first section. There is an interesting, and, I would judge, important discussion of the ascending scale of ' levels of being '—matter, life, mind, spirit—with which I suspect not all would agree. Dr. Yarnold suggests that to bridge the gap between the sciences and Christian theology scientists should accept three principles; (1) the external world is not self-existent but is created by God, (2) in studying the external world by the methods of the sciences, the human mind makes contact with the divine Mind: thus discovery and revelation are two aspects of the same encounter, (3) the fundamental laws of nature are what they are because God has so ordered the world which He has created. This may be a deceptive simplification: to some it may even seem to beg the question—but it goes to the heart of the matter and is a fruitful starting point for mutual understanding.

In the second section Dr. Yarnold gives a useful survey of the methods and developments of Biblical criticism which should remove many misunderstandings. He then gives a good deal of space to a discussion of the possibility or impossibility of miracles. This is for many scientists a crucial issue. Not every theologian would accept Dr. Yarnold's own position, but the issues are clearly stated and the arguments are fair.

The third part has not, in my opinion, the same value as the rest of the book and some of its sections give the impression of less careful thought and argument. It is important that it should be realised, as Dr. Yarnold rightly says, that " The Church through its members must take the trouble to understand the contemporary problems from within before presuming to speak in the name of the Lord ". I could wish that Dr. Yarnold had given himself more space to work out this understanding fully. As it is, the treatment of some of the ethical issues is inevitably somewhat superficial.

Yet this in itself may stimulate the thought and discussion in which scientists and theologians must engage together. This book serves a useful purpose in provoking thought. I hope that it will be read by many VIth form boys and girls and by university students. I hope too that it will find many readers among the clergy. It is a valuable and stimulating contribution to the ' conversation ' between the humble Christian believer and the honest agnostic which is so desperately needed in the spiritual crisis of our modern society.

ROBERT PETRIBURG.

Nuclear Reactor Physics. By RAYMOND L. MURRAY. (London: Macmillan & Co. Ltd., 1959.) [Pp. 317.] 30*s*.

The aim of *Nuclear Reactor Physics* has been to present an outline of the elementary theory of nuclear reactors with special reference to the derivation of design formulae for the nuclear engineer. This book does not discuss problems of heat removal, but concentrates on calculating the critical size of a reactor, the distributions of neutrons in the reactor, and how the reactor behaves when it operates, both in the steady state and also when this is disturbed.

Books on reactor physics and reactor theory are sometimes prefaced with an account of atomic and nuclear physics much of which, while interesting, is barely relevant, for the nuclear engineer can find it in many text books. The present author has resisted this temptation and expects familiarity with the necessary physics; he starts immediately with a short description of the nuclear reactor itself.

In his first chapter he introduces some elementary facts about reactors and discusses, with examples, how they may be classified according to type. He presents the idea of a critical reactor, in which the production of neutrons by fission is just equal to all the losses. The first problem faced by the reactor designer then is to calculate the critical size of the reactor and the distribution of neutrons within it, and to select the relevant parameters. Methods have then to be found of determining these parameters for a particular reactor.

The second chapter opens with definitions of neutron cross section and of neutron flux (which is the product *number of neutrons per unit volume* × *the neutron speed*). As the distribution of the thermal neutron flux in a reactor varies with position, the next problem is to find this variation. This is done by equating the flow of neutrons into a given volume of the reactor to the sum of the flow out of, and the losses within, this volume. At this stage approximations have to be made and the author takes the simplest acceptable formulation where all the neutrons are assumed to be travelling at the same speed, and after collision with a nucleus are equally likely to be scattered in any direction. In addition it is assumed that the neutron flux does not change violently with position. From this the fundamental equation can be derived, relating the size of a reactor of given shape with the distribution of neutron flux.

However, it is first necessary to consider the effect of the neutrons in the reactor which are slowing down, that is gradually losing energy. In a thermal reactor where the majority of fissions take place with slow moving (thermal) neutrons, there are in fact neutrons of all energies below fission energy. The neutrons produced in fission are travelling at high speeds and in a thermal reactor have their energy reduced by collisions with the nuclei of the moderator. From the mechanics of these collisions the variation of the neutron flux for neutrons of above-thermal energy can be deduced.

By combining the behaviour of the slowing-down and thermal neutrons the reactor equation can be formulated. In addition to the relation between the flux and critical size this elementary theory relates the size to certain characteristics of the arrangement of materials in the reactor. This combination is known as the buckling.

In addition the so-called Fermi Age equation is derived and this gives the distribution of neutron flux for all neutron energies in a system.

Even this simplified evaluation of critical size has its difficulties; so the author then considers an even simpler variation where all the neutrons in the system are supposed to have the same speed. This is known as the one-group neutron diffusion theory, and several examples are given of its applications, including the case of a reactor core surrounded by a reflector. Despite its simplicity the one-group theory gives satisfactory results for many problems of thermal reactors.

Most thermal reactors are of the heterogeneous type, that is, the fuel or fissile material is separated from the moderator in discrete units called fuel elements, associated with the cooling system of the reactor. The parameters involved in calculating the buckling of such a system are the fraction of thermal neutrons absorbed by the fuel (the thermal utilisation), the fraction of neutrons which escape capture by the resonances in ^{238}U (the resonance escape probability), the contribution of fissions by fast neutrons (the fast fission effect) and the slowing-down and diffusion lengths which are respectively concerned with the distances travelled by a neutron when slowing down and as a thermal neutron.

However, for some kinds of calculation the elementary one-group theory is inadequate so, in addition to a group of thermal neutrons as in one-group theory a second group representing all the slowing-down neutrons is introduced. The next chapter describes the use of two-group theory in the calculation of critical size, including again the problem of a reflected reactor.

In a critical reactor the neutron productions and losses just balance. However, a reactor is not always in this state; for example, it may be shut down, which means removing the neutrons at a much faster rate than they are produced so that the neutron flux falls. Alternatively, it may be required to increase the reactor flux which also means increasing the heat produced; such an increase has to be very carefully controlled in order not to reach unacceptable temperatures in the reactor. These variations constitute the time-dependent or kinetic behaviour of a reactor. The main problem considered is the variation of the total number of neutrons in a reactor consequent upon a sudden change the neutron-absorbing properties of the reactor. The important effect of the delayed neutrons in the time-variation of the number of neutrons is discussed at length, as also is the simplified theory in which all the delayed neutrons are assumed to have the same characteristics.

So far a reactor of constant characteristics has been discussed. However, changes take place in a real reactor when it is operated. Any change of total heat production changes the temperatures of the components, for example. Further, the fissile material is burnt up and probably—depending on the type of reactor—partly replaced by a different fissile material. Some of the fission products will absorb substantial numbers of neutrons.

The effects of temperature are discussed both generally and with specific reference to certain reactor types. However, although much of the book has previously been devoted to the heterogeneous reactor this chapter largely deals with homogeneous reactors (where all the components of the core are intimately mixed together). This seems an unnecessary omission as there is plenty of information available on temperature effects in heterogeneous reactors. In addition some of the problems which are discussed seem a little out of place in the book as they deal with reactor types which have not previously been mentioned.

Control of a reactor in a steady state is often achieved by maintaining the neutron balance by the insertion or removal of heavily neutron absorbing rods (control rods). The very elementary problem of a single control rod along the axis of a cylindrical reactor is dealt with by one- and two-group theory. Other effects which lead to changes on the reactor are then described. The first of these is the burn-up, in a reactor using uranium as fuel, of the ^{235}U and the ^{238}U isotopes with the subsequent production of plutonium isotopes. Control by a burnable 'poison' is briefly discussed, and also the important effect of poisoning by the build up of fission product ^{135}Xe. For some reason, no mention is made of the other important poison ^{148}Sm. Finally in this chapter there is a brief mention of the application of perturbation theory to nuclear reactors.

The two final chapters are on a different level from the rest of the book. They are mainly mathematical and deal with the transport theory for both mono-energetic and slowing-down neutrons, and finally with the extension of the neutron diffusion theory to more than two groups. The reviewer feels that these last two chapters could well have been omitted. The remainder of the book introduces elementary reactor theory to the nuclear engineer very well—with the limitations mentioned. It gives just sufficient theory for the reader to understand the derivation of the results quoted, but does not overburden him with mathematics. These two chapters are heavy going for the average design engineer. They will probably appeal to the physicist who is greatly interested in reactor theory, whereas the remainder of the book is really not intended for such a reader, who might be expected to consult other well established texts on reactor theory which are less specifically directed to the nuclear design engineer. J. F. HILL.

BOOK REVIEWS

Exploring the Structure of Matter. By Jean-Jacques Trillat, translated by F. W. Kent. (London: George Allen and Unwin Ltd., 1959.) [Pp. 214, plates 24, figures 92.] 30*s*.

What comes to mind when the words 'structure of matter' are used? Neutrons, protons, electrons; solid, liquid or gas? A 'solid' can include ice, metal, wood, flesh, and 'structure' may mean the grain of the wood, the difference between a snowflake and a hailstone, or the much finer periodic and symmetrical arrangements of atoms or molecules that go to form the very substance of ice, of diamonds, of a grain of metal or of a fibre of silk or thread of nylon. The structure of a thin film of a substance (say a bubble) is not necessarily identical with that of the same substance in bulk, nor is the arrangement of atoms within a solid precisely the same as that on its surface. Moreover, atoms and molecules are not resting quietly, but are continuously vibrating.

In order to investigate all these different aspects of the structure of solids, apart from those of liquids and gases, or the structures of atoms themselves, many different techniques are required and no one man is a master of them all.

The author of this book is a master of some of them, notably micrography by X-rays and electrons, and electron diffraction. He has, however, very little experience of X-ray diffraction techniques and refers to them only in a completely inadequate and usually slightly derogatory way. Thus on p. 181 of this translation he says . . . " whilst the basic data given by Röntgen photography (*sic*) can only confirm the molecular formulae that are established by chemical methods, the results obtained by electron diffraction enable the spacing between the atoms and the angle between their bands in the interior of the molecule to be measured exactly ". Shades of vitamin B12 and of penicillin! The facts are, of course, that X-ray diffraction and electron diffraction methods are complementary in the sense that the former is used for matter in bulk, the latter for films and surfaces, but the precision of the first is incomparably the greater, giving not only exact *intra-* and *inter-*molecular geometry but also the amplitudes of vibration of the atoms in various directions, except for very complex crystal structures. The author also states on p. 118 that it is impossible to determine, by X-rays, the positions of H atoms, thus showing that he has no knowledge of the beautiful method of differential Fourier synthesis now always used in the X-ray analysis of structures containing both light and heavier atoms.

In short, this is a one-sided book, not at all to be recommended for a balanced account of the subject implied by the title. Like the curate's egg, it is excellent in parts, and full of interesting details where the author is writing from his own personal experience, but with many gaps and downright errors where he is not.

The translation has evidently been made by someone who has taken great pains to read up the subject, but who is not familiar with it. Many phrases are used which are not those found in the scientific literature: the word 'diffusion' everywhere instead of 'scattering'; 'factor of atomic diffusion' (p. 121, etc.) instead of 'atomic scattering factor'; 'centre faced cube system' (p. 150) instead of 'face-centred cubic *lattice*; 'surstructure rays' instead of 'superlattice reflections'; 'monochromaticized' (p. 114) instead of 'made monochromatic'; 'shading method' (p. 84) instead of 'shadowing technique'. But what really shocked this reviewer was the use of *ues* instead of *e.s.u.* (p. 52), and of UX instead of Xu (p. 117). Did it not occur to the translator that units have different names, and initials, in French and in English? Characteristic wavelengths are given on p. 23 as Kx instead of Kα, an obvious printing error, but one that ought not to have been overlooked.

To sum up, the reviewer tried hard to obtain a copy of the original from some scientific library in London, in order to clear up certain obscurities in the translation; but was not successful. That itself seems to be a fair comment on this book, although it *is* good—in parts.

Kathleen Lonsdale.

The Neutron Story. By D. J. Hughes. (New York: Doubleday & Co. Inc., 1959.) [Pp. 58.] 195 cents.

This interesting little book is one of the Science Study Series, the primary purpose of which is " to provide an authoritative survey of physics within the grasp of the young student or layman ". The author, Dr. Donald Hughes of the Brookhaven National Laboratory, has accordingly tried to tell his readers about the neutron and its properties, about its uses in probing into the structure of matter, and about its practical applications, in general language with the minimum of technical terms and mathematics.

After introducing the neutron and explaining why it has become so versatile a tool in modern physics, the author gives a brief account of the nuclear theory of atomic structure, leading to the discovery of the neutron. He then devotes a chapter to Waves and Particles, necessary for the comprehension of much that is to follow. He proceeds to discuss the nature of the neutron and its relation to other ' fundamental ' particles, bringing in the neutrino and the meson, and gives the picture of its structure on the ideas now current. The next three chapters deal with the behaviour of the neutron in its passage through matter—its interactions with atomic nuclei, including sections on the production of radioisotopes, how the chemical elements were first formed, and present concepts of nuclear structure; then its interaction with atoms in crystals, mentioning its use in finding the positions of the atoms, especially of hydrogen atoms, and in studying the magnetic fields inside crystals; and the use of very slow or ' cold ' neutrons to give information about the vibrations of crystal atoms. The final chapter describes the chain reaction in uranium both in the bomb and the nuclear reactor, and concludes with some remarks on the possibility of the release of energy by the fusion of light nuclei.

It is no easy task to give such a wide survey of this rapidly advancing subject within the limitations imposed by the size of the book, about thirty-five thousand words, and by the use of non-technical language. There are several instances where the meaning is far from clear ; and those readers who remember the early days of nuclear physics may be somewhat surprised by some of the author's remarks on the history of those times. But these are small blemishes in a book which every reader, whether professional or amateur physicist, or layman, should find interesting and stimulating. In his preface the author claims that " the story has elements of mystery, profundity, and beauty, whose significance can be conveyed to those without technical training who are willing to put their minds to it ". I think this claim is justified. J. CHADWICK.

Theoretical Aerodynamics. By L. M. MILNE-THOMSON. 3rd Edition. (London: Macmillan & Co., 1958.) [Pp. 414.] 40*s*.

Professor Milne-Thomson sits apart from the rush and tumble of aeronautical practice and quietly surveys most, but not all, of the game. He picks out from the whole field of aerodynamics those parts, mainly concerned with calculating the forces on aerofoils and wings, which appeal to him as a mathematician and displays them with the same lucidity and excellent layout that characterized his earlier book, ' Theoretical Hydrodynamics '. The strength of the present volume lies not in an encyclopaedic coverage of the subject but in that what is done is, for the most part, done thoroughly. With the exception of the final chapter on supersonic sweptback and delta wings, the book has remained almost unaltered since the first edition (1948). It is pleasant to notice that the price too has remained unchanged. The new final chapter first appeared in the second edition (1952); the third edition differs from the second only in a few minor changes of wording and in that, rather oddly, a few new misprints creep in at each edition to offset those that are corrected. The book therefore includes a few war-time references but is mainly pre-war in origin—on p. 39 there is a very firmly dated illustration.

The author limits himself to a careful discussion of mathematical techniques, taking pains to provide his reader with all the necessary information for a full understanding of the steps in the analysis but not worrying over-much whether he has an equally clear grasp of why or whether the techniques have practical value. Thus there is an explanatory chapter on vector methods but no equally clear statement of the fundamental hypotheses and hardly any comparison with experiment. The publisher's comment printed on the dust-cover misleadingly asserts that " the general standard of the book is that of a first year university course ". The steps in the problems discussed and the many exercises set for the reader may well be within the comprehension of a first-year mathematics undergraduate but the approach is hardly suitable for a first-year engineer. An aeronautical engineer would only begin to appreciate the book in the latter half of his course and really value it only if he moved into mathematical research in fluid dynamics. At this later stage he would be challenged and titillated, rather than irritated and confused, by the unorthodox terminology, the lack of balance and the out-of-date aspect of some parts of the book.

How many aerodynamicists would nod wisely when told (p. 128) that " relevé is essential for the existence of a centre of lift " ?

The onlooker sees most of the game, but he does not hear all of the gossip between the players. It is not sufficient that *insentropic* has been replaced by *homentropic*. And the standard tropopause height of 36 093 ft is not without real significance; it is 11 km. Need there be four index references to a casual remark on typhoons ? On a higher level of criticism, it would seem preferable to miss out whole slices of a subject, as boundary layer theory and unsteady flow theory are omitted from this book, rather than to slip in a quite inadequate chapter on a subject such as aircraft stability. The best of the book, the part that makes it well worth getting and which in any case forms the bulk of the book, is the first fourteen chapters on the theory of incompressible inviscid flow past aerofoils and wings. Incidentally, the steady-flow explanation of the formation flying of geese (p. 207) is surely unconvincing. Is it not likely that the birds accept a leader and then keep clear of the wake of one another ? G. JACKSON.

The Electrical and Magnetic Properties of Solids. An Introductory Textbook. By N. CUSACK. (London, New York and Toronto: Longmans, Green and Co., 1958.) [Pp. xv + 428 with 205 figures and 25 tables.] 45*s*. net.

There are not many books suitable for university solid-state physics courses. Students have to rely frequently on library copies of expensive progress report or review type publications. Although such consultation is not to be discouraged, the average undergraduate prefers a moderately priced book which he can purchase himself. The author has attempted to produce such a book, but by limiting himself to electrical and magnetic properties, he does not cover all the requirements of a solid-state course. However, the standard reached in certain sections together with the many references quoted extends the usefulness of the book to beyond undergraduate level.

An opening chapter on the free electron gas model of a metal assumes a knowledge of Schrödinger's equation but includes sections on statistics which the reviewer feels would be found elsewhere in university courses. Surface phenomena are covered in chapters on work function and contact potential, thermionic and photo-electric emission. Early theories of conductivity are reviewed before a chapter on band theory introduces later conductivity theories, semi-conductors and ionic crystals. Four chapters cover the wide fields of dia-, para-, ferro-, antiferro- and ferri-magnetism. Unfortunately the chapter on superconductivity was written perhaps a year too early! Other topics are thermoelectricity, dielectrics, piezo- and ferro-electricity.

Useful accounts of relevant experimental work accompany the theoretical treatment and many recent experimental results are quoted. A glossary at the end introduces the student to a number of topics not covered in the main text. Within the limits he has set himself the author has succeeded in producing a comprehensive, modern work which will be welcomed by both students and teachers. J. H. PHILLIPS.

Properties of Matter. By F. C. CHAMPION and N. DAVY. Third Edition. (London: Blackie and Son, 1959.) [Pp. xvi + 334.] 40*s*.

In distinguishing classical physics from atomic physics we sometimes look back wistfully on how the former was once taught, and perhaps overlook the developments that have taken place in such an old subject as ' properties of matter ', and in particular how advances in atomic physics are affecting theories and interpretations in other fields. We have of course still a long way to go in solid state physics and in the physics of liquids before a completely different sort of text-book can be written. Meanwhile authors have the difficult task, while retaining the old framework, of trying to keep their books reasonably up to date.

This well-known and widely used book was first published in 1936, and a second edition with a fair amount of new material appeared in 1951. The additions in the third edition are slight, the important change being " not primarily the deletion or the addition of particular topics but the gradual orientation of the subject from a largely phenomenological treatment to atomic interpretations ". Some sections on capillarity have therefore been deleted and a short account of paper chromotography added; there is also a new section on the elastic behaviour of single cubic crystals. The other changes have mainly to do with diffusion in liquids and solids, with some reference to interpretations in terms of vacant atomic sites and interstitial atoms.

Many students and teachers must already have found the book clear and readable; its usefulness would be enhanced if more references were given and the lists of books at the end of some of the chapters extended and brought up to date. J. TOPPING.

Introductory Physics. By H. PRIESTLEY. (London: John Murray, 1959.) [Pp. xviii+516.] Library edition 45*s.*, class edition 37*s.* 6*d.*

Written for general education college courses in the United States, this is a book which should commend itself to teachers who are seeking suitable reading for non-specialist science classes at sixth-form level.

The author believes that the best way of understanding the scientific attitude is through the discipline of systematic study of one field, and the range and sequence are those of the usual elementary physics course. But he is also concerned with the scientist as a man, and the place of science in society; he describes the way in which the pioneer investigators approached their problems, gives many quotations from their original writings as well as references, and generally keeps the reader's attention on the human side of physical science.

It is an excellent and stimulating book, well written and on the whole well illustrated; and it should be welcomed thankfully. With this as the background to an experimental course, a teacher might make a good case for the time necessary to work through the whole; he could not expect to get this, but it would be easy to select the material for several terms of rewarding work on a more limited scale. G. R. NOAKES.

Technische Kunstgriffe bei physikalischen Untersuchungen. By V. ANGERER-EBERT. (12. völlig neubearbeitete Auflage.) (Braunschweig: Vieweg & Sohn, 1959.) [Pp. vi+458.]

It is now nearly a quarter of a century since I reviewed the third edition of von Angerer's little book, and I remember that on that occasion I made one or two remarks which I know were at once appreciated, and I was interested to see them incorporated in this twelfth edition. The latter is far more comprehensive than the one to which I have referred. Its format is quite different, its range much increased and, instead of some 200 pages of large type, we now have some 460 pages of close type with very many tables of data.

It is divided into four main parts: 1. Materials, their properties, preparation and use; 2. Special laboratory techniques; 3. Special apparatus and equipment; 4. Rules for the proper conduct of a laboratory. In addition, references are copious and there is an interesting list of German sources of supply.

I found the new volume very interesting. The tables of alloys and the appropriate heat treatments necessary to develop their peculiar properties are particularly notable; so, too, are the sections on the mounting of electrodes in glass, on high pressure apparatus, special dewar vessels and the production of magnetic fields.

It is strongly recommended as a convenient source of information. L. F. BATES.

Problèmes de Physique et de Chimie. By G. DÉVORÉ. (Paris: Vuibert, 1959.) [Pp. 372.] 3200 fr.

This is the second edition of a book containing about 110 problems in physics and 50 problems in chemistry, mostly culled from papers set by French regional examining bodies. These questions are of varying degrees of difficulty and are often quite lengthy (100–200 words) and in sections. The solutions which follow are not always extensive and their nature is shown by a letter which usually indicates the difficulty of the problem. Considerably more than half of the solutions which follow are complete; most of the remainder are abbreviated solutions in which the intermediate calculations have been omitted.

The physics problems have a more mathematical bias than is usual in similar British texts, and the choice of topics appears somewhat arbitrary, for some parts of general physics and mechanics are thinly represented. In this edition, the author has introduced a few problems giving two solutions, based on c.g.s. and m.k.s. non-rationalized units. A very useful introduction gives hints on solving problems, common symbols, units and a note on the calculation of errors.

The problems in chemistry are less advanced than those given in physics, and deal with practical aspects of thermochemistry, ionic equilibria, equilibria and kinetics in chemical systems, and quantitative analysis. W. A. SIDDALL.

Chimie Organique. By G. DÉVORÉ. (Paris: Vuibert, 1959.) [Pp. 432.] 5000 fr.

Any student of organic chemistry who has an elementary knowledge of French will enjoy reading parts of this work not only for practice in reading French science texts, but also to discover how the subject is presented. It is written for the use of students preparing for a specified State examination (approximately General degree standard) and forms an admirable introduction. The sub-title 'Les grandes fonctions simples' indicates the method of treatment under six sections.

The first section is a concise presentation of the general outlines of the subject, and of the methods used to isolate, purify and analyse an organic compound. In addition to the well established methods, the newer techniques of molecular distillation, counter current extraction and various forms of chromatography are all briefly discussed, and well illustrated by clear line diagrams. The basic principles of stereochemistry including methods of resolution are outlined and the section concludes with a chapter reviewing the use of physical properties as a means of determining structures. The author is to be congratulated on an orderly presentation of essential facts.

The succeeding five sections are equally well written and deal with hydrocarbons, alcohols and phenols, aldehydes and ketones, carboxylic acids, and finally nitrogen compounds.

W. A. SIDDALL.

Instruments and Laboratories. Quarterly. 92 rue Bonaparte, Paris, 6^e. 500 fr. each issue.

This bilingual journal has much to commend it. The seventh issue, covering January to March 1959, comprises two full-length articles on important new instruments, followed by short descriptions of other French instruments, written authoritatively by the companies responsible for them. As might be expected nucleonic devices figure largely, and the editor is on controversial ground in his classification code. What he terms measuring instruments, we should call 'detecting elements', and we should recognize about two hundred having important industrial uses as against his twelve. Nevertheless, the journal is a welcome addition in a field which is not too well cultivated and it is well worth the money. J. THOMSON.

Introduction to the Theory of Groups. By P. S. ALEXANDROFF. Translated by Hazel Perfect and G. M. Petersen. (London and Glasgow: Blackie & Son Ltd., 1959.) [Pp. 112.] 17*s.* 6*d.*

This book has been translated with a few minor alterations and additions from the German translation of the original Russian. In the foreword it is stated that the book has been written in the first place for the mathematically inclined pupils in the senior classes of grammar schools, and for those who teach them. In such a book there must be some connecting link between familiar ideas and the new material. Some knowledge of the concepts of a Set and Mapping is necessary before embarking on the Theory of Groups. The authors have realized this by discussing Sets in the appendix, and most readers will find the work easier if they study this first; it might profitably have been amplified and put at the beginning of the book. In the main chapters of the book itself the illustrations used to introduce the various topics are helpful and the sections on Permutation Groups and the Rotation Groups of geometrical figures do show how the new ideas can be applied. The grammar school pupil must be warned that he will have to be industrious; a great deal of new work is concentrated into a small space and there are many new technical terms to be learnt. The translation seems good and the style is concise and economical. The book itself is very well printed and produced.

E. S. KENDALL.

The Book of Experiments. By L. DE VRIES. (London: John Murray, 1959.) [Pp. 124.] 15*s.*

This interesting collection of simple experiments for children shows that much can be done with everyday apparatus, and will encourage an enterprising child to ask questions, So far, so good. Unfortunately, the author's answers are often the kind that a playmate might provide, and the book would be vastly improved by a careful revision in more adult terms. One would be interested to hear how the author managed to do experiment 119 (' Newton's Rings '), which is one of the more exacting optical demonstrations even with good apparatus. It is attractive enough, and as light reading will give a good deal more pleasure than the trivial commercial kind of children's book. But the type of person who is keen enough actually to work through it and try the experiments is the type that (however young) will expect some more scientific explanation of why they work—or, as in No. 119, why they don't.

G. R. Noakes.

Biographical Memoirs of Fellows of the Royal Society. Vol. 4. (London: The Royal Society, 1958.) [Pp. 360.] 30*s.*

This volume contains the obituary notices of 27 Fellows of the Royal Society who had died since the publication of the previous annual volume. Each article contains full biographical details, a list of publications and an appraisal of the work of the deceased.

Among them appear the names of three foreign members elected under the Statutes of the Society for their special eminence in their respective fields: Langmuir, of America General Electric Company fame; Störmer, a versatile Norwegian, chiefly known today for his work on aurora; and Wieland, prominent in the development of steroid chemistry.

Of the ordinary Fellows who constitute the bulk of the Society's members there are twelve deceased who specialized in the physical sciences and ten in biological or geological fields. They include Sir Francis Simon, a pioneer in low-temperature physics; Rowledge, chief of the design staff of Rolls Royce; Kennaway, best known for his discovery of carcinogenic chemical agents; and Lord Cherwell.

Finally the list is completed by two names well known in the political rather than the scientific field: Walter Elliot and Lord Waverley. They were elected to the Society under a special statute concerning persons who have rendered conspicuous service to the cause of science, though not themselves professional scientists.

The volume naturally contains much information on the progress of science during the lifetime of these individuals.

College Physics. By R. L. Webber, M. W. White and K. V. Manning. 3rd Edition. (New York, Toronto and London: McGraw-Hill Book Co., 1959.) [Pp. 640 + vi.] 58*s.*

A rigorous presentation of the basic ideas of general physics for students of science, engineering, pre-medicine, *and the liberal arts* is the authors' aim, and this third edition, should strengthen the appeal to the last category particularly. The introductory chapters on mechanics are good; and indeed about two-thirds of the book is devoted to mechanics and electricity—which are the parts of physics that demand the most careful presentation. In geometrical optics the wave treatment is used effectively. The mathematics of the text is simple, but more advanced proofs are given in the appendix. The general ideas and outlook of modern physics are explained, without too much detail, in the last chapters on relativity, quantum theory, atomic physics, solid state electronics, and nuclear physics. Each chapter has a well-chosen selection of questions and problems, most of the latter being straightforward numerical exercises. Production, finish, and illustrations (which include a portrait gallery of Nobel Laureates up to Frank and Tamm) are excellent.

The Thirteen Steps to the Atom. By Charles-Noel Martin. (London: George G. Harrap & Co., 1959.) [Pp. 256, plates 118.] 25*s.*

The thirteen steps are ' magnifications ', ascending in powers of ten, and perhaps rather more in number than even the imagination can count or need. The first six or so are represented by photographs obtained with optical, electron and field emission microscopes. Then come a number of emulsion, cloud-chamber and bubble-chamber photographs. The accompanying text is clear and accurate; and, considering that it is presumably meant for the general reader, sensibly condescending. Many of the pictures are of considerable technical interest, notably those showing electron diffraction, radiation damage taking place in bone, Menter's copper phthalocyanine work, and a ' fly's-eye ' reconstitution of an x-ray diffraction pattern. The standard of reproduction is high, and the size of the pictures is generous enough for ' visual-aid ' purposes.

Plasma research

by D. R. CHICK
A.E.I. Research Laboratory, Aldermaston Court, Aldermaston, Berks.

SUMMARY

The reaction power densities for the important nuclear fusion reactions are compared with the radiation losses due to Bremsstrahlung and cyclotron radiation. Some properties of an ionized plasma and the particle motions in such a plasma are discussed and methods of containing a plasma within magnetic confining fields are described. The present ideas on the stability of such confining fields, resulting from several theoretical studies, are outlined, and various means of heating the plasma are discussed. The main features of the experimental devices disclosed at Geneva in 1958 are compared, and the leading experimental results reported at the 1959 I.E.E. Convention on Thermonuclear Processes are reviewed.

1. INTRODUCTION

The nucleus of any atom is made up of a number of protons and neutrons which are held together by strong attractive forces. These nuclear forces vary in strength with the number of particles present in the nucleus and are a maximum for nuclei containing about 50 nucleons. Resulting from this, it is a property of nuclear structure that stable nuclei containing less than about 50 nucleons have a mass which is less than the sum of the masses of any two nuclei possessing, together, the same number of nucleons. A net gain of energy will thus, in principle, be obtained if we can build up such a nucleus by fusing together its constituent components.

The more familiar process of fission of very heavy nuclei owes its energy yield to the fact that the mass of the fissile nucleus is greater than the sum of the masses of the fragments after fission has occurred. Fission yields more energy per nuclear event than is available from fusion reactions. The energy available per gram of fuel will, however, be several times larger for the fusion process.

Fusion of hydrogen into helium was first observed by Oliphant *et al.* (1934). The reaction products were helium and a neutron moving apart from each other with very high velocity. Later work revealed the complete range of reactions of the light elements. Some of these reactions are listed in table 1.

Reactions (1) and (2) occur with roughly equal probability, reactions (3) and (4) are interesting because of their high energy yield and because they involve the reaction products of (1) and (2). Tritium, one of the important fuels, can be produced, but is very expensive, and all work so far has been done with deuterium.

(1) $\mathrm{D}+\mathrm{D} \rightarrow \mathrm{He}^3+\mathrm{n}+3{\cdot}25\ \text{MEV}$
(2) $\mathrm{D}+\mathrm{D} \rightarrow \mathrm{T}+\mathrm{p}+4\ \text{MEV}$
(3) $\mathrm{T}+\mathrm{D} \rightarrow \mathrm{He}^4+\mathrm{n}+17{\cdot}6\ \text{MEV}$
(4) $\mathrm{He}^3+\mathrm{D} \rightarrow \mathrm{He}^4+\mathrm{p}+18{\cdot}3\ \text{MEV}$
(5) $\mathrm{Li}^6+\mathrm{D} \rightarrow 2\,\mathrm{He}^4+22{\cdot}4\ \text{MEV}$
(6) $\mathrm{Li}^7+\mathrm{p} \rightarrow 2\,\mathrm{He}^4+17{\cdot}3\ \text{MEV}$

Table 1

Unlike the neutron-induced fission process, the interesting fusion reactions involve colliding nuclei both of which have positive charges. These electric charges result in a long-range repulsive electric force between the approaching nuclei, which predominates over the nuclear attractive forces until the two particles are separated by less than about 10^{-12} cm.

Thus only those charged nuclei approaching each other with high relative velocity or energy have a chance of a fusion collision. Even so, the repulsive force will cause many of the fast approaching nuclei to be turned aside from their original paths or 'scattered' before they are close enough to fuse.

The approach velocities required correspond to particle energies of some tens of thousands of electron volts, and limiting our interest for the moment to the DD reaction, Thompson (1957) has shown that the reaction cross section σ_{DD} (which is a measure of the probability of fusion) is given with reasonable accuracy, even at low bombarding energies, by the expression

$$\sigma_{\mathrm{DD}} = 198 W \exp(-46{\cdot}9/W) \text{ barns} \tag{1}$$

where W is the relative energy of the particles measured in kilo-electron volts. The barn, 10^{-24} sq. cm, is about the cross-sectional area of an average nucleus. The reaction yield is several MEV so that an energy gain is given by each nuclear fusion event.

Unfortunately, the scattering cross section $\sigma_e \simeq 2 \times 10^5 W^{-2}$ barns is very much higher than the fusion cross section (at 100 kev, $\sigma_e/\sigma_{\mathrm{DD}} \simeq 10^4$) so that if a target of deuterium is bombarded with deuterons the fusion yield is only about 10^{-5} of that required to produce a favourable energy balance.

Thus to obtain a useful fusion reactor, the nuclei, or ions, must somehow be retained within the system until fusion occurs in spite of being scattered hundreds or thousands of times before this happens.

2. Energy yields and losses

If, instead of bombarding a target with fast ions, we maintain a volume of deuterium gas at a very high temperature, each ion will undergo many collisions due to the thermal motion and if we can arrange to retain the scattered ions within the system for a sufficient time, the number of nuclear events can, in principle, be increased sufficiently to yield a net gain of energy. When speaking of the temperature of a gas it is convenient to take very roughly the figure that an ion of 100 ev energy has a temperature of a million degrees, an ion with 1 kev energy has a temperature of 10 million degrees and so on. The kinetic-theory expression for the average kinetic energy of a particle is $\frac{3}{2}\boldsymbol{k}T$, where $\boldsymbol{k}$ is Boltzmann's constant, and, more strictly, the particle temperature is 7730 degrees per electron volt.

We are thus concerned with contained systems of hot ions, and space-charge effects require that except over very small distances the fluid must be electrically neutral. Such an electrically neutral mixture of ions and electrons is called a plasma, and kinetic thermal equilibrium is quickly established between the ions and electrons.

To derive the power released in a gas we must consider the separate contributions by ions having the full range of velocities given by a Maxwell distribution of velocities, and as the cross section for the fusion reaction varies so enormously with the velocity the total power depends predominantly upon the ions of greater velocity.

In a deuterium plasma at temperature T°K the power P_R released per unit volume by fusion reactions is given by

$$P_R = \tfrac{1}{2}n^2(\overline{v\sigma})1{\cdot}6 \times 10^{-13}E_1 \text{ w/cm}^3 \tag{2}$$

where n is the density of deuterium nuclei per cm³, E_1 ($\simeq$4 Mev) is the energy released in one fusion reaction, and $(\overline{v\sigma})$ is the product of the relative velocities of the nuclei and the reaction cross section, averaged over the Maxwellian velocity distribution corresponding to the temperature T.

W. B. Thompson has computed the values of this averaged product over the range of temperatures of interest for the various fusion reactions.

The increase of power with mean gas temperature is not so dramatic as the increase of cross section given in fig. 1, but is, nevertheless, very high. Thus

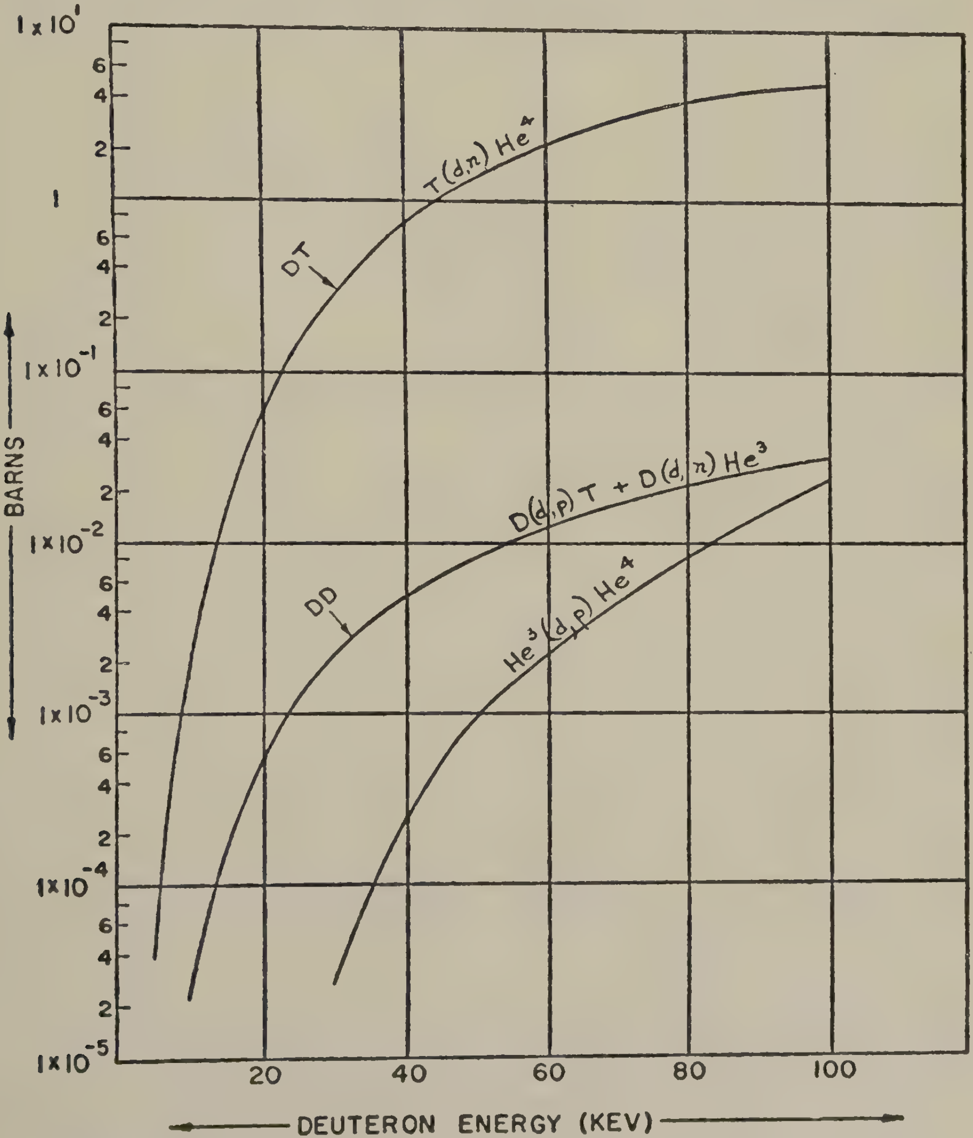


Fig. 1. Nuclear fusion cross sections as a function of particle relative energy.

from 100 ev to 1 kev the power rises by 10^9; from 1 kev to 10 kev the power rises by 10^3; and from 10 kev to 100 kev the power rises by 50.

Figure 2 shows how the power yield of a fusion reaction at a temperature equivalent to 100 kev (10^9 °K) varies with the density of the gas. The horizontal line is a typical power density of 100 w/cm³, which can be achieved in the uranium of a fission reactor. The density of 10^{15} ions/cm³ corresponds to a gas having a pressure of a fraction of a millimetre of mercury at room temperature.

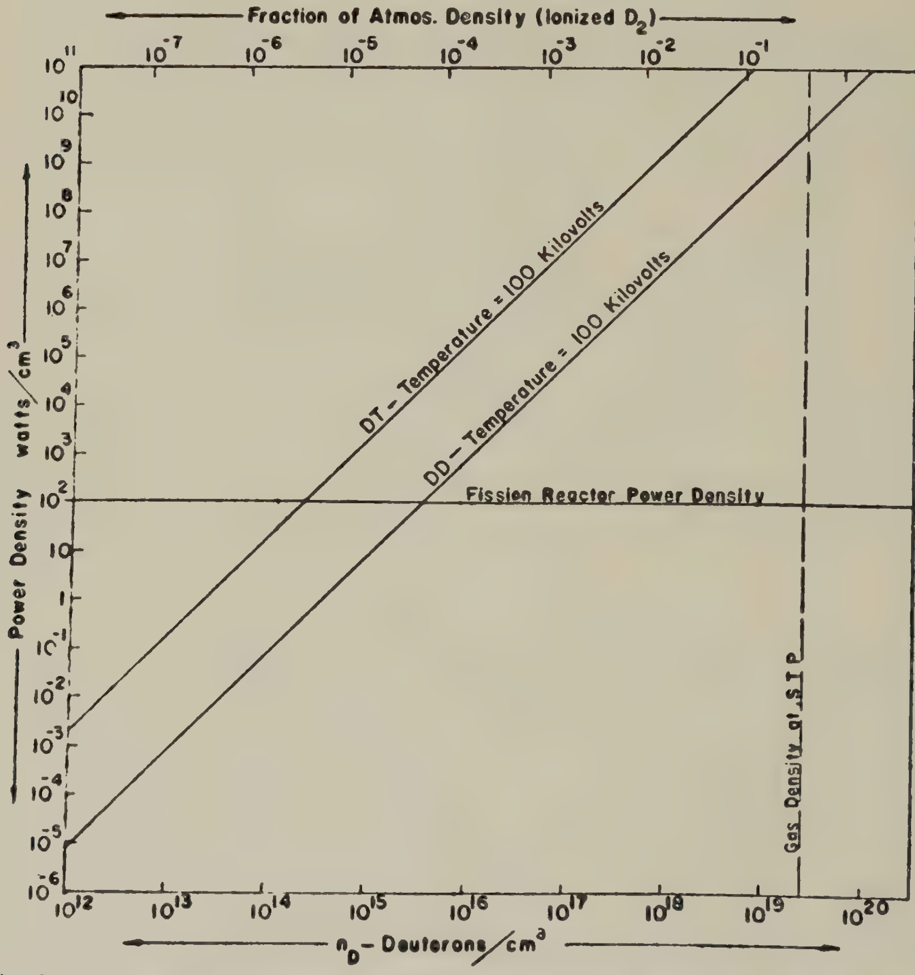


Fig. 2. DD and DT total reaction power density as a function of deuteron particle density at a temperature of 100 kev.

But processes in which energy is lost compete with the nuclear yields, and a reactor can only be successful if there is an excess of yield over loss. If we can achieve complete containment of the gas so that no charged particles reach the walls of the containing vessel, there will be no conduction or convection losses of energy, but losses by radiation remain. Now, the mean free path for internal absorption of any radiant energy generated in the gas is much larger than the dimensions of any conceivable vessel, hence equilibrium is not achieved between particles and radiation; so such a system does *not* behave as a black body, with its prohibitive radiation loss proportional to the fourth power of the absolute temperature.

Radiation arises from the scattering of electrons in the coulomb field of the nuclei. This radiation, called *Bremsstrahlung*, covers a broad frequency band extending into the x-ray region. Spitzer (1956) has given the power density P_B radiated as Bremsstrahlung for the case where the electrons have a Maxwellian velocity distribution corresponding to a temperature T_e as

$$P_B = 0{\cdot}39 \times 10^{-30} Z^2 n_i n_e (W)^{1/2}\ \text{w/cm}^3 \tag{3}$$

where W is the energy in kev corresponding to temperature T_e, n_i, n_e are the numbers of ions and electrons per cm³ and Z is the nuclear charge. This equation includes the term Z^2, and illustrates the extreme importance of maintaining the deuterium gas free from contamination by impurity atoms of high Z.

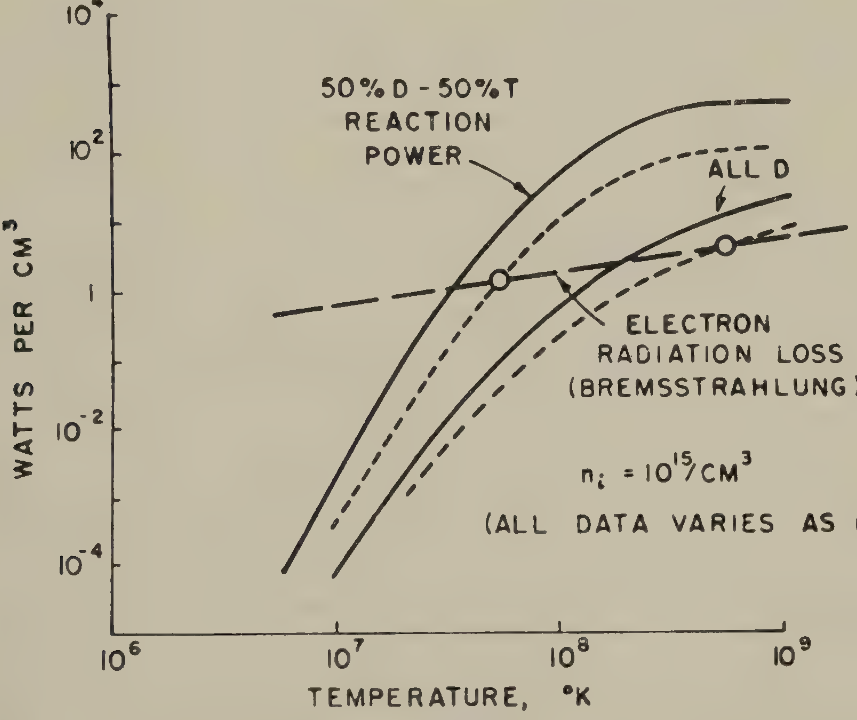


Fig. 3. Full-line curves show the total released power densities P_R for DD and DT fuel; broken line the electron radiation loss P_B. If there were no other losses T_c would be given in each case by the intersection of line and curve; the dotted curves show the power densities released as kinetic energy of the charged particles.

Figure 3 compares the fusion power P_R released by a gas of density 10^{15} atoms per cm³ in equilibrium at a temperature of 10^6 to 10^9 degrees with the radiative power loss P_B varying as the square root of the temperature; if there were no other losses the 'break-even' or critical temperature, where $P_R = P_B$, would be about 2×10^8 °K for deuterium and about 3×10^7 °K for a DT mixture.

There are other important factors to be considered in estimating the critical temperature. Jensen *et al.* (1958) point out that as the fuel is burnt in such a reactor, the reaction rate will drop as helium is built up and this will require that the critical fuel temperature must be increased to 6×10^8 °K for deuterium, or 6×10^7 °K for deuterium–tritium mixtures.

Secondly, the time t for which an ion is contained within the plasma before it is lost to the walls must be long enough for the chance of fusion to be significant; quantitatively this means that t must be greater than $1/n(\overline{v\sigma})$.

For the case of a plasma which is heated to a temperature T_c for a time t and is then allowed to cool (that is, t is the lifetime of the entire plasma) Lawson (1957) has shown that the product nt must be greater than 10^{14} for a useful power reactor using tritium/deuterium fuel and greater than 10^{16} for deuterium fuel. Thus with densities of 10^{15} the ions must have lifetimes in the plasma of 0·1 and 10 sec in the two cases respectively.

In addition to these effects there is a further radiation loss, the so-called magnetic or cyclotron radiation. As we shall see later, the ions and electrons, when acted on by a magnetic field, B, will execute circular paths around the magnetic field lines until disturbed by collisions. Since such orbits involve an acceleration normal to the motion, the electrons will radiate electromagnetic power $P_M = (4{\cdot}3 \times 10^{-25}) nWB^2$ w/cm³. If the magnetic field is essentially due to currents flowing in the plasma then

$$P_M = (2{\cdot}3 \times 10^{-32}) n^2 W^2/\beta \text{ w/cm}^3 \tag{4}$$

where β is the ratio of thermal to magnetic energy density and the temperature of the plasma W is expressed in kev and B in gauss.

The extent to which this is lost or reabsorbed in the system is uncertain. While a DT fuel can supply this additional loss, Trubnikov and Kudryavtsev (1958) have shown that a simple deuterium fuel must be heated to a temperature corresponding to about 50 kev and must have a thickness of several metres to yield a net energy gain.

3. The plasma as a conductor

If the hot gas is to lose energy only by radiation, protons and electrons must be prevented from reaching the walls of any containing vessel.

In the absence of restraining forces we can demonstrate that impossibly large apparatus is needed to contain the scattered particles.

Spitzer has shown that the mean free path for scattering is

$$\lambda_S \simeq 2 \times 10^{18} \,.\, W^2/n \text{ cm} \tag{5}$$

where W is in kev. Hence for a plasma of 10^{15} particles/cm³ at a temperature of 10^6 degrees the free path is some 20 cm. However, since the plasma is ionized and behaves as a conductor, it is possible to modify its behaviour by using electric and magnetic forces.

From a theoretical consideration of scattering in a hot plasma Cowling (1945) and Spitzer and Härm (1953) have derived an expression for the resistivity ρ, which increases with Z, the nuclear charge. For a hydrogenic plasma ($Z = 1$)

$$\rho = 2{\cdot}4 \times 10^{-6}/W^{3/2} \text{ ohm cm.} \tag{6}$$

Thus at 10^7 °K the gas resistivity is about that of copper; and this resistivity decreases rapidly as W increases. This implies that in a macroscopic sense currents can be caused to flow in the plasma and these currents can be made to interact either with their own magnetic fields or with externally applied magnetic fields. It is thus possible to arrange the fields and their configurations so that the interaction forces on the components of the plasma tend to prevent the plasma from losing the energy to walls.

4. PARTICLE MOTIONS IN THE PLASMA

It is important to realize that the flux density B of the magnetic field can result from the superposition of externally applied fields and magnetic fields due to the currents in the plasma. In general, for all cases except the pinch pevices the main contribution to B comes from currents flowing in external fixed conductors.

In these devices the currents flowing in the plasma may only modify the values of the externally imposed fields in a minor way. If this is so, the behaviour of such a device can be deduced from the motion of the individual particles in the externally applied field.

If we consider a low-density plasma such that effects due to pressure gradients and collisions between the particles are negligible, both resistivity ρ and current density J are very small, and the product ρJ is a small quantity of the second order. With these simplifications the individual ions can to all intents and purposes be treated singly. Further, suppose that there is no electric field, and that the magnetic flux-density B is uniform in space and constant in time. Then a particle with velocity v in a direction making an angle θ with that of B experiences a force $Bev \sin \theta/\mathbf{c}$ always at right angles to its path (here B is in gauss and e in absolute e.s.u., with v in cm sec^{-1}), and thus describes a circle. Equating this force to the centripetal '$mr\omega^2$' force, the angular velocity is given by

$$\omega_c = eB/m\mathbf{c} \qquad (7)$$

the well-known cyclotron equation. Here v has disappeared from eqn. (7); but there is still the $v \cos \theta$ component parallel to B, and while all particles of the same e/m gyrate with the same ω_c, each has its own drift velocity $v \cos \theta$ along the field lines. The orbits of the particles are thus helices parallel to the field with ions and electrons rotating in opposite directions with frequencies in the ratio (m_e/m_i) respectively. For a given rotational energy W_q in kev we have

$$Br_c = 106 \left(\frac{m}{m_e}\right)^{1/2} W_q^{1/2} \qquad (8)$$

where m is the mass of the particle and r_c is the particle orbit radius. At a temperature of 10^6 °K and in a field of 10^3 gauss the radius for the positive ion is 2 cm and the frequency of revolution is 700 kc/sec. For an electron of the same energy the radius is 0·03 cm and the frequency is 3000 Mc/sec.

If we now apply an electric field E_p parallel to the magnetic field the particle will spiral down the line of magnetic force with unchanged radius, but the helical orbit will become one of increasing pitch, positive ions will be accelerated, in the direction of E_p, and electrons in the opposite sense. If an electric field E_q is applied perpendicular to the magnetic field an ion executes a circular motion of constant angular velocity, normal to the magnetic field lines and additionally the centres of these orbits have a drift velocity of E_q/B at right angles to E_q and B. The positive ions and the electrons drift in the same direction.

There are two other orbital motions of great interest where there are magnetic fields whose magnitudes change with position. One concerns ions in a toroidal chamber in which ions are travelling down the torus and there is a magnetic field also down the torus in the same direction. Such a field, created, let us say, by a solenoidal winding placed on the torus, will be more intense at the inner wall than at the outer. A particle, therefore, executing a helical orbit around the magnetic lines spends part of its time in regions of higher field strength and part in regions of lower strength so that its orbit drifts at right angles to the magnetic gradient. In this case the positive ion drifts in the opposite direction to the electron, so that charge separation tends to occur.

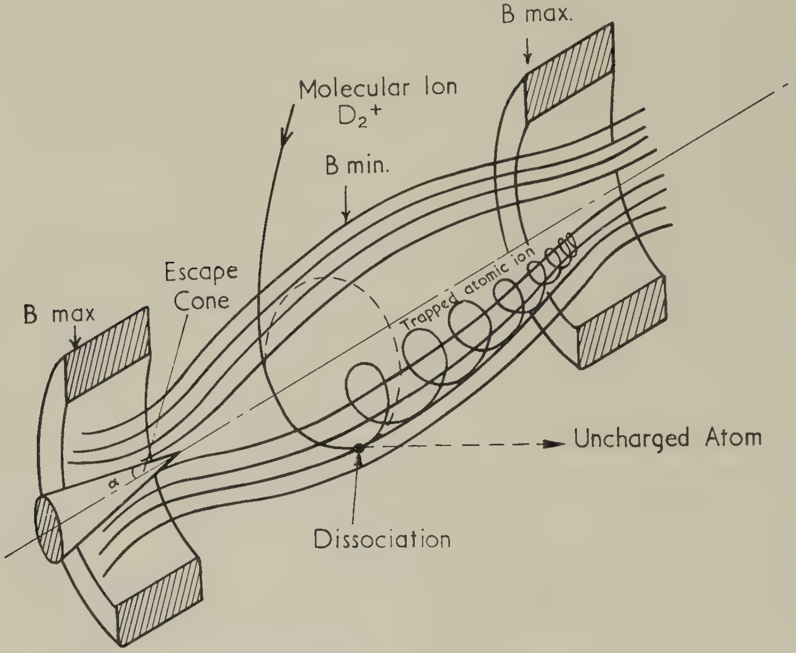


Fig. 4. Typical ion trajectory in a magnetic mirror field.

The other orbital motion of interest concerns ions travelling along lines of force which become bunched together. Such an ion has a fraction of its energy W_p in the direction of the drift, and the balance in circular rotational energy W_q around the line of magnetic force to preserve the conservation of angular momentum of the ion. This rotational component of energy must depend on the strength of the magnetic field so that

$$\frac{W_q}{B} = \frac{e}{2mc} \times (\textit{angular momentum}) = \textit{magnetic moment.} \qquad (9)$$

Schlüter (1957) has shown that the magnetic moment is constant and this is still true even when B changes by a small fraction in the time taken for a particle to complete one cyclotron orbit. As the particle drifts down the magnetic line of force ($E_p = 0$) to a region of greater magnetic field density, while its total energy must remain the same the fraction represented by a rotational energy increases, and there may come a time when all its energy is rotational and it will then be brought to rest in a longitudinal sense. Any perturbation will then set it drifting back in the opposite direction. This means of containment by reflection is termed a magnetic mirror and is the basis of Post's (1956) magnetic mirror concept. Several American machines and the Russian OGRA machine make use of the magnetic mirror principle to keep ions inside the two ends of the so-called magnetic bottle (fig. 4).

Post has shown that the plasma contained in such a magnetic bottle can be heated by allowing the magnitude of B to increase slowly with time. The plasma is then compressed by the increasing field and is heated adiabatically. Schlüter further suggested heating by varying B cyclically with time. Thus if B increases, W_q increases also, as shown in table 2. If after the field increase there is sufficient time, the particle will share its increased energy by scattering with other ions; some of its rotational energy will then be converted to translational energy. If then the magnetic field declines to its original value, the rotational energy of the ion will fall without affecting its translational energy, but the net change is an increase of total energy. Let this happen in cyclical fashion and we have a magnetic pumping action which heats the gas. It is the possibility of achieving the *inverse* of this process whereby nuclear energy released in a hot plasma causes it to expand against and compress the confining magnetic field, which underlies the hope of direct conversion of nuclear to magnetic energy.

	B	W_q	W_p	$W = W_q + W_p$
Increase	1 10	2 20	1 1	3 21
Collisions intervene Decrease	10 1	14 1·4	7 7	21 8·4
Collisions intervene Increase	1 10	5·6 56	2·8 2·8	8·4 58·8
Collisions intervene Decrease	10 1	39·2 3·9	19 6 19·6	58·8 23·52 and so on

Table 2. Magnetic Pumping.

5. CONTAINMENT

In order to isolate the plasma from any material walls we must use either gravitational, or electric, or magnetic forces. Gravitation requires such a large mass that it is suited only for the stars. Static electric fields would be extremely difficult to establish since even the regions surrounding the plasma are conducting, and would also tend to cause charge separation. Most attempts at plasma containment involve magnetic fields, either steady or varying slowly with time. Some consideration has, however, been given to systems employing radio-frequency fields; but the powers required appear impracticable. The containment problem is particularly difficult because we are dealing with a high-temperature ionized gas which has some unusual properties. A nearly perfect conductor made of an elastic material is perhaps the simplest model of the plasma. Collision events are comparatively infrequent, hence resistance or friction-like losses are low, and violent oscillations and instabilities can occur. Charge neutrality exists within the plasma and any charge separation tends to be suppressed. There is a diffusion of particles towards regions of lower pressures and the rate of diffusion is inversely proportional to B^2. Magnetic forces on the particles are normal to B and to their own velocity; thus the particles are held to the field lines and gyrate about them.

Consequently if the magnetic field is contracted the plasma contracts with it and we are able to move the plasma by moving the field, while the reciprocal property also exists such that if the plasma moves it carries the magnetic field with it. Since the resistivity though approaching zero does not entirely vanish, there is a slow diffusion between field and plasma.

Electric fields normal to B, magnetic field gradients, or curvature of the magnetic field lines, allow the particles to drift across the lines of force of the containing magnetic field. Any time-changing fields set up counteracting currents in the plasma which tend to keep the field from penetrating, and if the field is set up first and the plasma then heated, the field tends to be trapped inside the gas.

Using some severe simplifying assumptions, the magnitude of the field needed for containment can be estimated. Here it helps to remember that the ions are really behaving in two guises; as far as random motions go, they are like ideal-gas molecules, exerting mechanical pressure, and with average kinetic energy $\frac{3}{2}\boldsymbol{k}T$ (where here T can for the moment be taken to mean the temperature that would be calculated if they were indeed merely molecules in thermal equilibrium). But for organized motions, they are an electric current and produce a magnetic field. If at any point in the plasma there is an axial current density J_z at right angles to B_θ, a force proportional to $J_z B_\theta$ and directed at right angles to both J and B, i.e. radially inwards, will exist. Qualitatively we see that such a force can balance a pressure gradient, and summing the effect over the radius of the current channel we find that such a current channel can contain and compress the kinetic gas pressure of the plasma. During the process ohmic heating and adiabatic compression increase the plasma temperature. If we consider a simplified model of a current I_z flowing along the axis (the z direction) of a long cylinder of radius a, immersed in an externally applied axial magnetic field of flux density B_z, we see that the axial current itself provides a θ directed magnetic field, and the resultant field lines in the plasma will form helices; while due to the compression B_z within the plasma differs from that outside it.

It can be shown that for such a model the following equation, known as a 'generalized Bennett relation' holds :

$$I_z^2 + \frac{a^2}{4} B_z^2{}_{\text{ex}} = 2N\boldsymbol{k}\bar{T} + \frac{a^2}{4} B_z^2{}_{\text{in}} \tag{10}$$

where $B_z^2{}_{\text{ex}}$ is B_z^2 external to the plasma, and $B_z^2{}_{\text{in}}$ is the average value of B_z^2 inside the plasma. N is the average number of all charged particles per unit length and $\bar{T}$ is the mean temperature averaged over all charged particles in the discharge. For the case of no externally applied field this reduces to the form

$$I^2 = 2N\boldsymbol{k}\bar{T}.$$

Equation (10) can be rewritten as follows:

$$B_a^2 = \bar{B}_z^2{}_{\text{in}} + 8\pi\bar{p}_a \tag{11}$$

where B_a is the value of the total field at radius a just outside the plasma, and if there are no particles outside this radius, then $\bar{p}_a$ is the average pressure within the plasma. Thus the plasma pressure $nk\bar{T}$ that can be contained varies as B^2, and for a plasma of 10^{15} particles/cm³ at a temperature of 10^8 °K the field must exceed 26 000 gauss.

For a given value of B_a, which is a measure of the power supplied, either to external windings or directly to the discharge, the maximum value of $\bar{p}_a$ is achieved with zero $B_{z\,\text{in}}$. As we shall see later, such a system is highly unstable. Both theoretical and experimental work has suggested that magnetic fields within the plasma would alleviate instabilities. But such internal magnetic fields will tend to prevent the discharge constricting, and will reduce $\bar{p}_a$ and hence $\bar{T}$ for a given density and power input.

In devices of the Stellerator type where the discharge is initiated in a strong axial field, so that $B_{\text{in}} \simeq B_{\text{ex}}$, only small values of $\beta = \bar{p}_a/(\textit{magnetic energy density})$ can be achieved, unless the discharge can be made initially with high conductivity so that the B_z guide field is excluded from the plasma at all times.

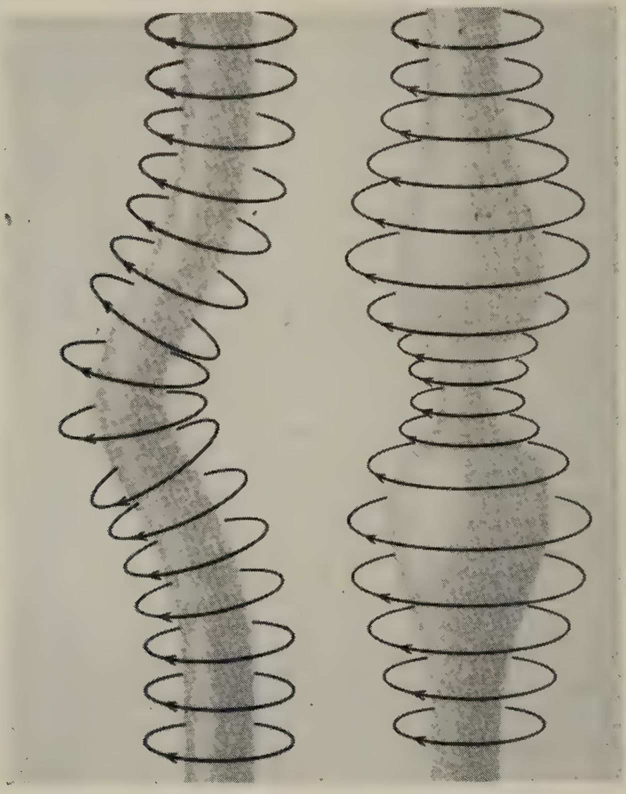
6. Stability

The central problem in all experimental approaches is that of the stability of a plasma immersed in a magnetic field. The problem has a special significance when most of the magnetic field is created by, and is 'anchored' to, intense currents flowing in the mechanically flexible plasma.

Studies of the pinched discharges soon showed that a highly constricted column of hot gas is unstable against forces which distort its shape. It is easy to see that this is so on physical grounds. A current flowing in a straight wire is accompanied by magnetic lines of force which form rings in planes perpendicular to the wire, with their centres on its axis. A small local disturbance of the wire will result in an increase in magnetic field on the concave side of the disturbance. The result of this is to increase the initial disturbance. Theoretically the column may have an infinite number of modes or forms of instability and two important forms are shown in fig. 5.

If an axial magnetic field fills the tube initially, it will be trapped and compressed by the highly ionized gas travelling inwards. This trapped field provides a tension of $B^2/4\pi$ along the lines, giving stiffness to the plasma which tends to be anchored to the field lines.

It may be seen intuitively that a moderate longitudinal magnetic field should provide some degree of stabilization. If an axial magnetic field fills the tube initially, it will be trapped and compressed by the highly ionized shell of hot gas travelling inwards. These trapped magnetic lines form a back bone for the discharge. The stabilizing effect of a longitudinal magnetic field applied either inside or outside the pinch column has been analysed by a number of workers (Kruskal and Schwarzschild 1954, Shafranov 1956, Tayler 1957, 1958). Theoretical predictions say that the very fast growing $m=0$ instability is suppressed by a moderate longitudinal magnetic field. The $m=1$ or corkscrew instability then tends to predominate. Theory predicts that this instability can be controlled within limits if the discharge is contained in an external conducting shell. The analysis requires that for stability the plasma must not be pinched into an area smaller than about 10 per cent of its original cross section. These theoretical predictions arise from a model in which currents are assumed to flow in an infinitely thin skin on the surface of the plasma and the plasma is assumed to be enclosed in an infinitely long and perfectly conducting straight tube. There is as yet no theory of discharge stability for a toroidal-shaped plasma.



$m=1$ $m=0$

Fig. 5. Instability modes of a pinched plasma.

Rosenbluth (1958) has treated the case of current flow in a surface layer of finite thickness and he suggests that instabilities will exist within such layers; the unstable perturbations predicted may well be damped by the viscosity of a real plasma.

Suydam (1958) has shown that a cylindrical plasma with diffuse boundaries between the stabilizing axial field and the confining self-magnetic field cannot be stable unless

$$\frac{8\pi}{B_z^2}\left(\frac{dp}{dr}\right)+\frac{r}{4}\left(\frac{1}{\mu_1}\cdot\frac{d\mu_1}{dr}\right)=0 \qquad (12)$$

where $\mu_1 = B_\theta / rB_z$ and p is the plasma pressure. Field configurations satisfying the above criterion may still be unstable.

Finally, Laing (1958), also using the work of Bernstein *et al.* (1957) and imposing stricter conditions than used by Suydam, has derived distributions of the magnetic fields which will with certainty lead to stable confinement of the plasma. The stable discharge proposed by Laing contains a central, an intermediate and an outer region. The axial current flows in opposite directions in the inner and intermediate regions and the net current is zero. In these two regions the plasma pressure increases outwards, while in the outer region no axial current flows and the pressure falls to zero. The axial component of magnetic field is distributed in such a way as to satisfy the requirement of pressure balance. It is difficult to see how to achieve the required field configurations in practice, particularly in toroidal geometries.

A state of kinetic equilibrium is assumed in the theoretical work leading to these predictions, but frequently it is necessary to collapse the discharge which originally filled the tube to achieve such equilibrium. Thus accelerations of the plasma are often involved, and Berkowitz *et al.* (1958) have shown that acceleration inwards causes an additional destabilizing effect while acceleration outwards enhances the stability.

7. HEATING

Of the several methods of heating the plasma the simplest is Joule heating by the currents producing the confining field. The power input per unit volume, $J^2\rho$, becomes small at high temperatures since $\rho = 4{\cdot}2 \times 10^{-6}/W^{3/2}$ ohm cm. Additionally, the fact that ρ must be finite in order to provide any Joule heating, means that interparticle collisions occur. As pointed out by Bickerton (1959) this results in the spatially separated axial and azimuthal fields required to ensure stability of a pinched plasma diffusing into each other. The initially sharply separated orthogonal field arrangement has a higher magnetic energy content than that prevailing after diffusion has occurred and this energy difference heats the plasma sheath to approximately one-half of the 'Bennett' temperature, T_b, which is defined by

$$2n_0 \boldsymbol{k} T_b = B_0^2/8\pi \tag{13}$$

where n_0 and B_0 are the initial ion density and magnetic field strength respectively. Thus the stability and Joule heating requirements of the self-pinched discharge are largely logically contradictory; collisions are needed to cause Joule heating, but finite resistivity causes field diffusion and tends to destroy the stability.

The temperature of the plasma can be increased by compression. The energy input derives from the work done by the magnetic force experienced by the current in the magnetic field. If the collapse rate is slow, the magnetic forces are in equilibrium with the kinetic pressure of the plasma at each stage of the compression, and hence the plasma is heated adiabatically and reversibly.

The plasma can also be heated by making the initial collapse velocity very high so that irreversible work is done on the gas by the compressing forces. In such systems, requiring very large electric fields, the behaviour of so-called 'collision-free' shock waves is of great interest. In such shocks a single particle is supposed to interact with other particles through the macroscopic electric and magnetic fields resulting from charge separation and current flow. The time scale for the passage of shocks is such that normal binary collisions between particles can be neglected—hence the term 'collision-free'. This process has the advantages that the energy input is not limited by resistivity of the plasma, and the energy is transferred initially to the ions rather than, as in Joule heating, to the electrons.

Niblett (1959) has studied a rapidly collapsing discharge in a system in which the confining force arises from an azimuthal compressing current associated with an externally supplied axial magnetic field. Such a system is similar to the Scylla devised by Elmore *et al.* (1958) and is really like the more familiar pinch configuration but with the rôles of B and J interchanged in space. Both of these systems are extremes of the general case in which plasma current and confining field may have components in both the axial and azimuthal directions.

Allen (1957) has discussed the properties of shocks produced by the motion of a current sheath in one dimension and, for a hydrodynamic one-dimensional model, Niblett shows that the collapse velocity, u, of such a current is

$$u=\left(\frac{3}{32\pi}\frac{E^2}{n_i m_i}\right)^{1/4} \tag{14}$$

where n_i, m_i are the number density and mass of the positive ions in the undisturbed gas.

Assuming a complete ionization and equi-partition of energy among the particles, the temperature T achieved by the plasma is given by

$$T=\frac{1}{8}\left\{\left(\frac{2}{3\pi}\right)\left(\frac{m_i}{n_i}\right)\right\}^{1/2}\frac{E}{\boldsymbol{k}(1+Z)}. \tag{15}$$

Thus, for a very large energy source of negligible internal impedance, which implies a constant current, the temperature of the gas is increased by the collapsing current sheet in direct proportion to the electric field developed across the plasma. The establishment of the very large loop voltages required ($\simeq 10^6$ v/turn) in a toroidal apparatus is a matter of considerable difficulty and no experimental work has yet been reported on shock heated toroidal pinches.

Lastly, the method of heating by cyclically varying the magnetic field as suggested by Schlüter (1957) and illustrated in table 2 has been applied to the Stellerator and could be used with other apparatus.

8. Experimental equipment

Fry (1959) has reviewed the various experimental approaches to the problems of achieving useful fusion reactions and he has summarized the properties of the devices described at the 1958 Geneva Conference (table 3).

Fry broadly classifies the experiments as open or closed line systems, depending on whether the magnetic lines of force of the containing field do or do not close on themselves within the vacuum chamber. Thus the Stellerator and Astron devices are classed with the toroidal pinch experiments as are the linear pinch devices when end losses are neglected.

The open line systems include the Livermore and the Russian mirror machines; the Oak Ridge molecular-ion injection device D.C.X.; and the azimuthal current discharge Scylla, which is similar to the device described by Niblett. The rotating plasma devices ‘Homopolar’ and ‘Ixion’ are treated separately.

As we have seen from our discussion on containment and heating there are major differences in the operation of the devices grouped in this way. For example, the toroidal pinch machines provide the major part of the containing forces by virtue of intense currents flowing in the plasma, whereas in the Stellerator most of the confining magnetic field is provided by currents flowing in external material coils; some of the stability problems are similar, although the much smaller plasma currents interact less violently with the containing field

Main classification	Sub-division	Devices	Location	Characteristic dimensions (cm)		Discharge current or magnetic field strength	Plasma density (ions per cub. cm)	Characteristic time (seconds)	Electron temperature (°K)	Ion temperature (°K)
				Torus bore	Torus diameter					
Closed magnetic lines	Toroidal stabilized pinch	Uppsala† Torus	Sweden	28	130	10^{6} amp				
		Saclay torus	France	8	78	5×10^{4} ,,	10^{16}	10^{-4}		
		Alpha	U.S.S.R.	100	320	2×10^{5} ,,	No official data, but believed similar to Zeta			
		Zeta	U.K.	100	300	2×10^{5} ,,	10^{14}	10^{-3}	$1\text{–}5\times10^{5}$	$\sim5\times10^{6}$ By Doppler broadening of impurity light
		Perhapsatron S.4	U.S.	14	70	3×10^{5} ,,	5×10^{15}	10^{-5} g	$\sim10^{5}$	$\sim10^{6}$ By magnetic pressure balance
		Gamma pinch	U.S.	10	60	3×10^{5} ,,	10^{16}		$\sim5\times10^{5}$	
		Sceptre	U.K.	30	110	2×10^{5} ,,	10^{15}	10^{-4}	$\sim10^{5}$	$\sim3\times10^{6}$ By Doppler broadening of impurity light
		Moscow torus	U.S.S.R.	48	125	2×10^{5} ,,	4×10^{14}	2×10^{-5}	$1{\cdot}5\text{–}3\times10^{5}$	
				Tube bore	Tube length					
	Linear pinch	Moscow linear pinch	U.S.S.R.	40	50	$\sim10^{6}$,,	7×10^{17}	10^{-6}	$0{\cdot}5\rightarrow$	$1{\cdot}0\times10$ From piezo-electric pressure measurement
		Uppsala linear pinch	Sweden	30	60	4×10^{5} ,,				
		Munich linear pinch	Germany	20	50	4×10^{5} ,,	$\sim10^{16}$	10^{-6}		$\sim10^{6}$ (estimated)
		Colombus II	U.S.	10	30	8×10^{5} ,,	5×10^{17}	10^{-6}		
		Colombus S.4	U.S.	13	60	$2{\cdot}5\times10^{5}$,,	$\sim5\times10^{16}$	$\sim5\times10^{-6}$	10^{5}	
		Colombus T.1	U.S.	15	600	7×10^{4} ,,	10^{15}	10^{-3}	3×10^{4}	10^{4}
		Maggi	U.K.	15	28	5×10^{5} ,,	$\sim3\times10^{17}$	10^{-6}		
		Saclay linear pinch	France	28	100	2×10^{5} ,,				
		Tri-axial pinch	U.S.	10	100	$1{\cdot}3\times10^{6}$,,	$\sim5\times10^{16}$	5×10^{-6}		
		Screw-dynamic	U.S.	10	20	$2{\cdot}5\times10^{5}$,,	$\sim5\times10^{16}$	10^{-6}		
	Stellerators	B_1	U.S.	5	450	30 kilogauss	3×10^{13}	10^{-3}		
		B_2	U.S.	5	600	30 ,,				
		B_3	U.S.	5	600	50 ,,				
		B65	U.S.	15	500	20 ,,	10^{13}	10^{-3}	5×10^{5}	
		C	U.S.	20	1,200	50 ,,	Not yet completed			
	Astron	Pilot model†	U.S.	100	1,000	1·6 ,,	10^{12}		1–3 MeV electron injection	
		Final power reactor†	U.S.	200	2,000	40 ,,	7×10^{14}		50 MeV electron injection	
Open magnetic lines	'Mirror' or adiabatic trap devices	DCX	U.S.	50	50	10 ,,			600 kev molecular ion injection	
		OGRA	U.S.S.R.	140	2,000	5 ,,			200 kev molecular ion injection	
		FELIX	U.S.	45	270	25 ,,			10–15 kev atomic ion injection	
		High compression	U.S.	15	100	44 ,,	10^{14}	10^{-2}	10^{5}	10^{7}
		SCYLLA	U.S.	7	7	38 ,,	$\sim10^{17}$	$\sim10^{-6}$		
	Rotating plasmas	IXION	U.S.	24	86	10 ,,	10^{14}	10^{-3}		
		Homopolar	U.S.	25	10	13 ,,	10^{16}	10^{-5}		

†Design figures only.

Table 3.

pattern. A basic point of similarity between the Stellerator and the mirror devices is that the ratio of kinetic pressure nkT, to the magnetic energy density required is small compared with that possible in pinch devices. The methods of heating the plasma also differ. Pinch machines rely essentially on Joule heating, the Stellerator uses in addition so-called 'magnetic pumping', while some of the fast linear pinch experiments have attempted to utilize shock heating.

Much theoretical work has been done on the stability of the Stellerator. The results indicate that stability will be achieved in a confining field configuration such that individual lines of force cannot be pushed aside or caused to interchange with each other, or at least that such changes result in an increase in magnetic energy and hence require the plasma to do work.

These requirements lead to field configurations in which the field line at a radius r has both θ and ϕ components and traces a helical path around the torus. The components are adjusted so that the line makes many revolutions tracing out a 'magnetic surface' before closing on itself. As suggested by the last term in the Suydam criterion (eqn. (12)) this idea is extended so that at a different radius $r+\delta r$ the helix angle is different and is in general some function of radius. The field pattern is then interlaced rather like the core of a multi-strand electric cable in which the successive layers of conductors have a different 'wavelength'. The field lines are locked in the sense that radial movement is difficult and such a configuration is said to have 'magnetic shear'.

While the Stellerator and the open line class of devices have a small value of $nkT/(B^2/8\pi)$ they have a further basic feature due to their field arrangement. Because the field lines do not close on themselves in the vacuum, particles whose velocity vectors lie within a cone of half angle α, where $\sin\alpha = B_{\max}/B_{\min}$ will escape from the mirrors. This is a major difficulty in the presence of collisions since, given time, there is a reasonable chance of all the particles in the plasma being scattered into this cone. Thus in general mirror type devices are seriously affected by scattering and it is necessary to reduce this effect to achieve a useful energy gain.

Since the scattering cross section is proportional to T^{-2}, mirror machines must operate at about ten times the temperature required for closed line systems; the problem of heating the plasma is approached somewhat differently. While adiabatic compression and 'magnetic pumping' techniques are used, emphasis is placed on the plasma being created at a high temperature in the first place. In most such machines this is attempted by injecting beams of atomic or molecular ions with kinetic energies ranging from 10 to 600 kev. The systems are arranged so that the resulting atomic ions are trapped in the mirror fields, the excessive directed kinetic energy being randomized by collision processes to provide a hot plasma.

The Astron is a sophisticated variant of the mirror geometry in which Christofilos (1958) hopes to achieve a field pattern in which the lines are closed on themselves in the vacuum chamber. The field pattern is produced in part by currents in an external solenoid as in the Stellerator. Then using an intense pulsed source of high energy electrons and a travelling magnetic wave method of injection, a cylindrical annulus of relativistic electrons (with speeds approaching c) called the *E-layer* is built up. The current in the E-layer reduces and finally reverses the field inside itself creating the desired pattern of closed lines. Neutral gas is now admitted. This is ionized and heated by the relativistic electrons.

The final version of the Astron is envisaged as a steady state device with periodic replenishment of the 50 MEV electrons forming the rotating E layer and continuous injection of neutral fuel gas. There are still doubts about the stability of the E-layer with 1–3 MEV electrons.

9. EXPERIMENTAL RESULTS

Bickerton (1959) and Ware (1959 a) have discussed the main experimental results reported on the pinch devices. These discharges are confined by their magnetic fields and are stable in the gross sense that they are kept away from the chamber walls. Magnetic probe measurements however show high frequency fluctuations of the fields with amplitudes about 10 per cent of the average values. These probe measurements also show that the plasma currents flow throughout the current channel with little tendency to flow in thin sheets or skins separating distinct field regions. Electron temperatures deduced from many differing measuring techniques appear to lie between the limits $1{\cdot}5 \times 10^5$ and 5×10^5 degrees. Surprisingly the impurity ions seem to have temperatures in the range $0{\cdot}5 \times 10^6$ to 6×10^6 degrees. (In each case here temperature is understood to be reckoned on the approximate basis of $1\,\text{ev} = 10^5\,^\circ\text{K}$, and the two sets of figures for electrons and ions simply mean that the two kinds of particles are not in thermal equilibrium with each other.) The plasma loses its energy to the walls in times of the order of 60 to 100 microseconds. The detailed mechanism of this loss is not understood and its elucidation forms a major part of current research.

Soft X-rays down to 2 kev energy are observed and these appear predominantly at those points where magnetic lines cut the inner wall. The energy content of these X-rays is insufficient to account for the known loss; however, the magnitude of the radiation flux below 2 kev is unknown. Colgate *et al.* (1958) and Ware (1959 b) have suggested that the energy loss is due to runaway electrons of about 2 kev energy reaching the walls. Ware points out that this supposition would also explain the high ion drift velocities observed.

Most of these results and suggestions will require confirmation by much careful experimental work of a detailed nature. Harwell are planning an experiment aimed at achieving skin currents and hence sharply separated fields to improve stability. This experiment, called I.C.S.E., will minimize runaway electrons by operating at the high densities of about 10^{18} particles per cm length. This will require a large increase in the initial energy store, while a very fast rate of rise of current demanding large loop voltages ($= 10^6$ v/turn) will be necessary to achieve the thin current skins.

The devices of the Stellerator type, listed in table 3 were described and discussed in detail at Geneva by Spitzer and his colleagues.

Most of the experimental work has been concerned with the initial ohmic heating stage. The time taken for the plasma to reach the walls has been observed to be several orders of magnitude shorter than the theory predicts, and moreover this time, instead of being proportional to the square of the magnetic field strength, appears to be proportional to the square root of that strength.

A large Stellerator installation known as Model C is nearing completion at Princeton. Costing about ten million pounds it is expected to begin operation in 1960. Experimental results reported from 'open line' devices, while encouraging, also indicate future difficulties. Post (1958) has demonstrated that a mirror device with $\beta = p/(B^2/8\pi)$ of 0·08 is stable, but he has emphasized that neither the theoretical predictions nor experimental results are sufficiently far advanced to predict stable plasmas at the temperature and pressure necessary for a reactor.

In the Livermore 'high compression' experiment plasma densities of $10^{14}/\text{cm}^3$ were achieved by means of thousand-fold increase of field.

It was found that the half-value containment time was of the order 1 millisecond with energetic components being observed to remain as long as 20 milliseconds. Electron energies were measured and corresponded to an electron temperature of about 15 kev or 150 million degrees. Under the conditions used, the ion mean energies could not have been as high as the indicated electron temperature. With different conditions ion energies of 2–3 kev were observed.

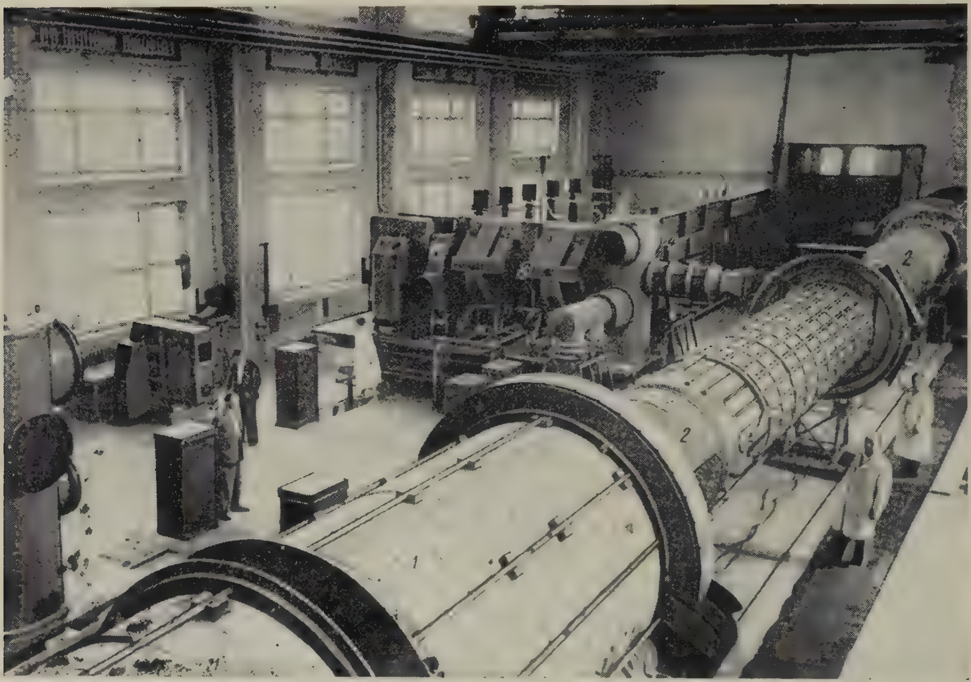


Fig. 6. OGRA, the U.S.S.R. mirror machine.

The Russian OGRA and the American D.C.X. mirror machines propose to build up a hot plasma by injecting fast charged particles. They rely on changing the value of e/m of the particles after injection so as to trap them in the system. The very large OGRA machine, illustrated in fig. 6, designed to achieve a background pressure of 10^{-8} mm Hg, utilizes this principle. Molecular ions (H^+_2 or D^+_2) with energies up to 200 kev will be injected transversely into the machine in such a manner that if they remained uncharged they would travel a distance of 1 km before returning to the injector. It is hoped that their dissociation and consequent trapping in the machine will occur through collisions with the residual neutral gas. Balancing the favourable process of dissociation against the unfavourable one of charge exchange, it can be shown that the build-up of plasma in the machine will only take place if the injector current exceeds certain critical values. It was claimed that for the OGRA machine these were 200 mA for H^+_2 ions.

The D.C.X. machine (Bell *et al.* 1958) built at Oak Ridge in Tennessee also uses the principle of high-energy molecular ion injection followed by dissociation. It differs from OGRA in that dissociation is achieved by passing the injected ions through a carbon arc column which runs longitudinally through the machine, shown in fig. 7. Since molecular ions passing through the column are dissociated with efficiencies of greater than 70 per cent, it is not necessary to provide for long molecular ion trajectories as in OGRA. As before, the competing processes of dissociation and charge-exchange set limits on the injector current and the background gas pressure if a substantial plasma is to build up in the machine. At present 300 microamps of 600 kev molecular ions are injected and the background gas pressure is 10^{-7} mm Hg. With this apparatus the containment of single particles for 10 milliseconds has been achieved.

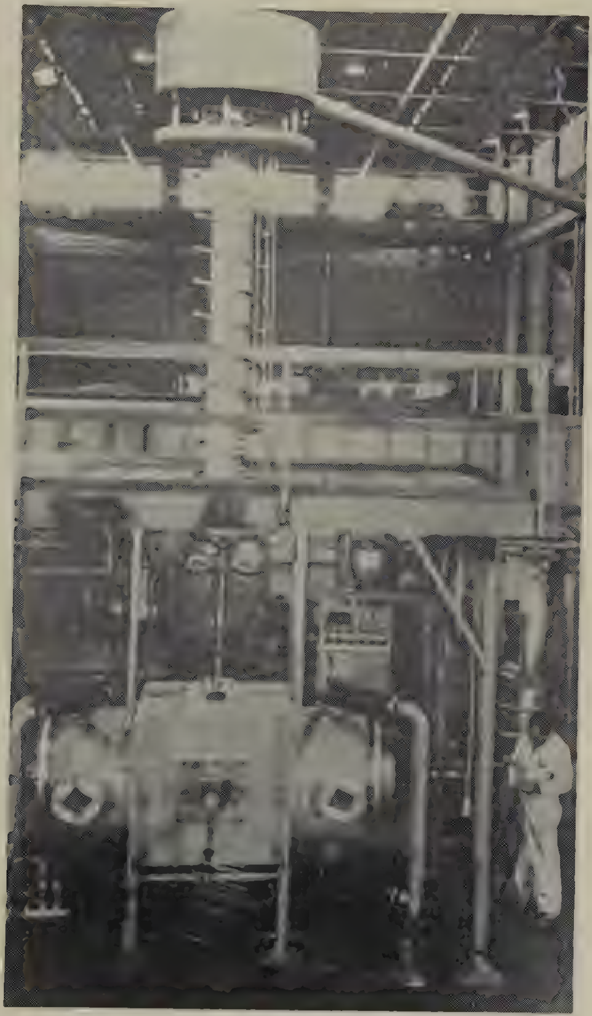


Fig. 7. The Oak Ridge D.C.X. machine.

10. Direct conversion

Many years of patient and difficult work seem to be required to study and understand the behaviour and physics of intense plasmas. In the light of such knowledge we must reassess the possibilities of achieving a net useful energy yield in a man-made device. If such an assessment indicates that the basic physics criteria can be met, the many engineering problems, including that of establishing the required field configuration, must then be resolved.

Nevertheless, human intellectual impatience is such that, assuming all this is done, consideration is already being given to the next step of improving the efficiency of conversion of fusion to electrical energy. Linhart (1957) suggested that it should be possible to obtain some part of the nuclear energy directly in an electrical form. Jukes (1959) and Bickerton and Jukes (1958/9) have discussed this proposal, which could lead to some saving of heat exchanger and turbine capacity required in a thermonuclear power station. Liley (1959) has treated the discharge and its associated equipment as a complex circuit network in which the parameters such as inductance are functions of geometric and thermal coordinates. By this means he gives a description which takes account of the non-rigid nature of the conducting plasma. This work points out the close inter-relation between the thermal and electrical states of the discharge and indicates how changes in the geometrical configuration modify these states. Taken in conjunction with Schlüter's suggestion that many of these relations hold when the magnetic fields are changing slowly with time, we have a picture of such a changing field acting as a moving wall or piston compressing and heating the plasma and supplying the radiation losses.

If we remember that the plasma, if heated beyond the critical temperature, is itself an energy source from the fission process, we have the required properties for a direct conversion device. In electrical terms, the discharge behaves as a variable inductance coupled through the transformer action to the primary winding and the external circuits. When the discharge is contracting its inductance is increasing and electrical energy is drawn from the supply to provide this magnetic energy and additionally some kinetic energy in the plasma.

If as a result of this increased thermal kinetic energy the plasma temperature is increased beyond 'ignition' it becomes itself an energy source resulting in a further temperature increase and an increase of current.

The hotter pinched discharge with its increased magnetic energy density now expands adiabatically against the confining field; its inductance increases, and the energy is fed back into the external circuit. Since the expansion occurs at a higher current more magnetic energy is fed back than was taken out.

Bickerton and Jukes have analysed the case of a self-pinched discharge adiabatically compressed about tenfold by time-modulating the pinch current. A sinusoidal modulating waveform yields direct conversion efficiencies of only about 10 per cent and, as we have seen, pure deuterium fuel is unlikely to reach 'ignition' temperatures. With a deuterium/tritium fuel mixture, only that part of the fusion energy (about 20 per cent) released as kinetic energy of the charged particles can be converted directly to electrical energy. A conversion efficiency of about 75 per cent appears possible if the pinch current can be modulated as a square wave, the plasma spending about 2 per cent of the time of each cycle above the ignition temperature. This will require peak currents of 20 million amperes in a discharge with a density of 10^{18} particles per cm length. Since most of the fusion yield ($\simeq 80$ per cent) is in the form of energetic neutrons which are not retained in the plasma, a large part of the electrical output of any future thermonuclear power station must be converted indirectly using heat exchangers and turbines. The partial direct conversion system suggested yields about one-third of the electrical output of a steady state device with similar temperatures and density values. However, comparing the two systems with equal electrical output, the partially direct conversion saves about one-third of the heat exchanger and turbine capacity required at the cost of the circuits needed to modulate currents of the order of ten million amperes.

Lastly, because of the variation with temperature of the velocity–cross section product, the direct conversion system described would be inherently unstable in the thermal sense.

11. Conclusion

Of the several experimental approaches being pursued, none is in a state to justify attempting even a prototype device, aimed at achieving the break-even temperature. Thus before any pilot zero energy thermonuclear reactor can be considered a large amount of basic information on plasma behaviour is needed. In particular, an intensive study is required to elucidate the nature of the instabilities occurring, and to understand the part that they play in the rapid loss of energy from the plasma. The experimental attack might well be an attempt to synthesize in small-scale apparatus discharges more nearly satisfying the severe stability requirements indicated by the present somewhat unsatisfactory theoretical position. Experimental results with more stable devices should help to clarify the important energy loss mechanisms. Radiation loss mechanisms and the effects of fuel burn-up will need detailed attention as will many of the very difficult engineering problems.

There is no particular reason for favouring any one of the experimental approaches in hand; they are all necessary studies of plasma physics. The pinch is the cheapest way of containing the plasma, but is unstable, while the mirror machines require much higher temperatures, and the ion injection devices must operate in spite of charge exchange difficulties.

Research work in the United Kingdom has been essentially concerned with the pinch devices. In order to build up our knowledge of plasma behaviour over the widest possible range of experimental conditions, there is a need to broaden the scientific approach at the present stage of relatively small-scale experiments.

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A history of neutrons and nuclei. Part 1

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1. Introduction

The history of science is concerned at least as much with discarded hypotheses as with accepted cosmologies: at least as much with man's stumblings towards a rational view of the world as with the precise aspect of that view at any one time, least of all the present. At the present time the word ' neutron ' has a perfectly definite connotation: it is the name given to the sub-atomic particle ' discovered ' by Chadwick in 1932—the particle whose existence had been postulated by Rutherford twelve years previously. But long before 1920 the word had been used speculatively by physicists with other meanings less precise. Much the same story might be told of the word ' nucleus '. Since 1913, in discussions of atomic structure, this word has been used without ambiguity of reference. But the word ' nucleus ' was used by Faraday in 1844, and it was used frequently—even the term ' atomic nucleus ' was used—by Larmor, in various publications, from 1898 to 1902. During the following decade it occurs variously, but always speculatively, in the writings of others.

It is the object of the present review to trace the history of the usages of the word ' nucleus ', and of the neutron concept, against the background of general ideas in atomic physics over the past hundred years. The word ' neutron ' is certainly to be found in the literature of the subject as far back as 1899.

2. Neutrons and ether

William Sutherland (1859–1911) is remembered chiefly for his formula for the temperature variation of gaseous viscosity (1893). The idea that the molecules of a gas might approach one another more closely at high relative velocity than at low was, in his day, a novel and adventurous idea; it represented an advance from the billiard-ball picture of the molecule; no-one has since questioned its essential validity. A Scotsman by birth, Sutherland spent his active life in Australia, where, for more than thirty years, as an arm-chair physicist, he had ample time for speculation. An obituary notice in *Nature*, over the initials J.L., says of him: " His writings were copious in all problems connected with molecular physics. . . . His generalisations were, indeed, so numerous that it was often a difficult task to estimate their value ". If the writer of the notice was, in fact, Joseph Larmor, the author of *Aether and Matter*, as is here supposed, it is possible that he had in mind one series of papers in particular—a loosely integrated series beginning with a paper entitled ' Cathode, Lenard and Röntgen rays ', published in the *Philosophical Magazine* in March 1899. It is in this paper that the word neutron first occurs; at least I have found no earlier instance of its use.

When the paper in question was written, the implications of Thomson's views on the nature and origin of the cathode rays had not been generally assimilated, and Stokes's hypothesis that x-rays arise in the rapid retardation of the cathode particles in matter was, by comparison, one stage farther from general acceptance by physicists. It is beside the point that these views have, in the event, survived misunderstanding and criticism essentially unchanged until today; in 1898 they were still under criticism, and Sutherland was one of their critics. He seems to have misunderstood Thomson's views, thinking that they implied " that atoms are split up into particles having different charges from the atomic charge in electrolysis ". To him, however, Johnstone Stoney's electron (1874) was a natural entity, not just a natural unit for the measurement of electrical charge. So he postulated free electrons, both positive and negative, having charge and finite size and inertia of electromagnetic origin. And he wrote: " If the electrons are distributed through the aether, we must suppose that in aether showing no electric charge each negative electron is united with a positive electron to form the analogue of a material molecule, which might conveniently be called a neutron ". He supposed that cathode rays originate from the ' splitting up of neutrons ' in the ' exhausted tube ', and metallic conduction from ' the local dissociation of the neutron into electrons '. He proposed ' to identify the Röntgen rays with the internal vibrations of our electrons ' (though I see no good reason why he selected the electron rather than the neutron of his scheme for this role). He considered the phenomena of the passage of Röntgen rays through matter, and in this connection distinguished between the constituent electrons of neutrons and the constituent electrons of atoms. He wrote: " we should expect the electrons forming the neutrons in the aether to be set vibrating by our waves. . . . But when the waves get amongst the electrons associated with the atoms, and set them vibrating internally, there is called forth . . . the damping action already spoken of. One of the probable results of such an action would be the setting of the acting and reacting atom and electron into relative motion, so causing the absorbed Röntgen energy to appear as some form of radiant energy congenial to the atom and electron. In this way our waves could give rise to fluorescent . . . effects . . . If an electron absorbs enough of the energy of our small waves, it may be set into such vigorous motion as to escape from the atom with which it is acting and reacting, and appear as a free electron . . .". Again, " The velocity of the Röntgen rays should be of the order of that of light: we cannot assert that it should be equal to that of light, because to waves of so short a length the neutrons may act as if they loaded the aether . . .".

Regarded in retrospect these views of Sutherland appear little more than fantasy; though there is a certain vague intuition of later ideas, there is no empirical necessity about his fundamental concepts such as a strict adherence to the accepted methodology of science must be held to require of the theorist, except in moments of inspiration. And Sutherland was no inspired theorist, certainly not consistently so throughout his ' numerous generalizations '. But it was an age of bewilderment in physics, and he continued to elaborate—and to complicate—his fanciful picture of molecules and atoms, of electrons and neutrons, wholly innocent, it would seem, of the thought that some day it must meet the challenge of direct quantitative test, or pass into limbo. In 1902 he was repeating: " the neutron and electron are the molecule and atom of electricity "—and he was setting up a cumbersome, if picturesque, symbolism to represent them: "As it would be convenient to have symbols . . . I propose to use the musical types ♯ for the + electron and ♭ for the − electron, and ♮ for the neutron . . ." Two years previously he had written, in a paper, on ' The molecular constitution of water ', " each negative H_2O ion [contains] two negative electrons, whereas the molecule H_2O contains a negative and a positive electron that equilibrate one another within the molecule. The formation of $(H_2O)_2$ consists then in two H_2O molecules exchanging electrons . . .": by 1902, considering the problem of valency, he was postulating neutrons within the individual atoms and molecules—" the halogens are heptads as well as monads . . . each halogen atom contains three pairs of ♯ and ♭ as neutrons, or as doublets . . .", " the special neutron in the steam molecule probably has a greater moment than that in aether ". In the previous year he had written a thirty-page paper on ' The cause of the structure of spectra ', and had concluded: " Our analysis has brought us to a fundamental constant angular velocity A for all electrons round the centre of any atom of any element. The electrical apparatus which produces all spectra is the same. In free aether the positive and negative electron revolving with angular velocity A round their centre of inertia form what I have proposed to call the neutron . . . Near an atom the two parts of the neutron have their motions controlled mechanically by the atom in the manner just discussed in accounting for Rydberg's Laws ".

No one else followed him into the intricacies of his speculation, but during the first years of the century Sutherland was not a lone romantic amongst physicists: his malady was partly of the age in which he lived. Walther Nernst (1864–1941), who lived to receive a Nobel prize for chemistry in 1921, as a young man wrote a textbook entitled *Theoretical Chemistry* which was to acquire an international reputation. The preface to the first edition was dated 1893. Ten years later, in the preface to the fourth edition, Nernst wrote " Though the phenomena [of radioactivity] are still very obscure I have attempted to present in a new chapter, ' The Atomistic Theory of Electricity ', the development of electron theory, which is more and more seen to be a powerful extension of atomistic method ". In this new chapter there occur these statements: " we must assume the existence of *positive and negative elementary particles*. . . . Thus we have to assume two new univalent elements whose atoms exert no, or very little, Newtonian attraction on the other elements. . . . We shall use the signs ⊕ and ⊖ as symbols for these [new] elements. . . . It is a question of much importance whether a compound of the positive and negative electrons (⊕⊖=neutron, an electrically neutral massless molecule) really exists; we shall assume that neutrons are everywhere present like the luminiferous ether, and may regard the space filled by these molecules as weightless, non-conducting, but electrically polarisable, that is as possessing the properties which optics assumes for the luminiferous ether ". All this is very much the mixture as before—including the clumsy symbolism, though no reference is made to Sutherland's writings, and his name does not appear in the index to the volume.

The malady of the age with which we are dealing was in large measure an inordinate affection for outmoded ethers. It is true that this obsession had deep roots in the past. " Newton said that one who believed in the possibility of attraction between distant bodies without an intervening medium must be a fool " (Hopkinson 1902); again, " Faraday was deeply influenced by the axiom, or if you prefer it, dogma that matter cannot act where it is not " (Thomson 1904). Later, in the preface to *Modern Views of Electricity* (1889), Lodge had written " The existence of an ether can legitimately be denied in the same terms as the existence of matter can be denied, but only so. . . . Few things in physical science appear to me more certain than that what has so long been called electricity is a form, or rather a mode of manifestation, of the ether . . .". From Lodge, indeed, the germ of the neutron concept of Sutherland and Nernst may well have been derived, for in a lecture on ' The ether and its functions ', delivered at the London Institution on 28 December 1882, Lodge had said " What I have to suggest is that positive and negative electricity together may make up the ether . . .".

But those are earlier views: let us look more closely at the decade of our main concern. In 1900, from his retirement at Netherhall, the ageing Kelvin (1824–1907) was setting down in majestic prose his passing fancy in the form of an ether model for electrons: " I now cannot resist the temptation to speak of efforts which occupy me to find proper assumptions for . . . atoms of electricity, which, following Larmor, I at present call electrons. . . . Thus a *positive electron* would be an atom which by attraction condenses ether into the space occupied by its volume; and a negative electron would be an atom which, by repulsion, rarefies the ether remaining in the space occupied by its volume. . . . This closely resembles Aepinus' doctrine . . .". And he concluded: " I confess that I now feel hopeful of seeing solved some of the other formidable difficulties which meet every effort to explain electric insulation and conduction, and electromagnetic force, and the magnetic force of a steel magnet, by definite mechanical action of ether ".

(In the following year all was changed. Kelvin produced a new version of a one-fluid theory in 'Aepinus Atomized ', his contribution to the jubilee volume presented to Johannes Bosscha of Haarlem: " My suggestion is that the Aepinus' fluid consists of exceedingly minute equal and similar atoms, which I call electrions ". Early in the development of this suggestion he wrote: " It is possible that the differences of quality of the atoms of different substances may be partially due to the quantum-numbers of their electrions being different . . .". A startling speculation, but then Kelvin's use of words was often strange and startling; there was, indeed, no anticipation here of quantisation in the Bohr-Sommerfeld sense—and in any case there was the tell-tale idiosyncratic ' i '.)

So we come, finally, to the views of Larmor and Thomson, senior and second wranglers, respectively, at Cambridge, in that vintage year, 1880. At the turn of the century Thomson not only regarded the ether as a physical reality, but, bringing to bear on Faraday's concept of lines of force the powerful geometrical insight of a latter-day Newton, he had developed an elegant theory of tubes of force, and these, too, he regarded as real entities. As first Silliman Memorial lecturer at Yale, in 1903, he told his audience : " The view I wish to put before you is that . . . the *whole* mass of any body is just the mass of ether surrounding the body which is carried along by the Faraday tubes associated with the atoms of the body ". And again, " Let us then take as our primordial system an electric doublet, with a negative corpuscule at one end and an equal positive charge at the other, the two ends being connected by lines of electric force which we suppose to have a material existence. For reasons which will appear later on, we shall suppose that the volume over which the positive electricity is spread is very much larger than the volume of the corpuscule. The lines of force will therefore be very much more condensed near the corpuscule than at any other part of the system, and therefore the quantity of ether bound by the lines of force, the mass of which we regard as the mass of the system, will be very much greater near the corpuscule than elsewhere . . . the mass of the system will practically arise from the mass of bound ether close to the corpuscule ".

Thomson worked out the consequences of this view in great detail, and, as third Adamson lecturer at the University of Manchester in 1907, he gave some of his conclusions: " Then the heat given out when the atoms of different elements combine will be equal to the diminution of the mutual electrostatic potential energy of the atoms combining, and therefore by what we have said will be a measure of the diminution of the etherial mass attached to the atoms; on this view the diminution in the etherial mass will be a mass which moving with the velocity of light possesses an amount of kinetic energy equal to the mechanical equivalent of the heat developed by their chemical combination . . . a gramme of radium gives out during its life an amount of energy equal to $6{\cdot}17 \times 10^{16}$ ergs . . . the atoms in a gramme of radium must . . . therefore have associated with them an etherial mass of between one-eighth and one seventh of a milligramme . . . ".

The malady of the age of which we have been writing was awaiting that solvent of all the old substantial ethers, Einstein's special theory of relativity, but its solvent action was slow; as we have just seen, two years after the publication of Einstein's theory Thomson's attachment to his ether was not diminished—out of it, moreover, he had conjured something very close to that baleful and yet liberating motto of our nuclear age, $E=mc^2$.

Of Larmor it is not necessary to write in great detail here. His ether was a subtle and sophisticated variant of the more substantial ethers of his contemporaries: Whittaker has written of it (1951): " This point of view enabled Larmor's theory to withstand subsequent criticisms based on the principle of relativity, which shattered practically all rival concepts of the aether ". But Larmor's ether had its material aspects, nevertheless. In 1904 he wrote: " If, then, matter is for physical purposes a purely aetherical system, if it is constituted of simple polar singularities or electrons, positive and negative, in the Maxwellian aether. . . . As electrons are already held to be a reality on various grounds, theoretical and experimental, it would appear therefore that there is much to be said for a benevolent attitude to the proposition that all the interactions of matter, so far as the laws of physics and chemistry extend, are to be described as phenomena occurring in and through the aether . . .".

Perhaps the epilogue of the story should be with Hopkinson, whom we have quoted already. He expanded some remarks he had made at the Glasgow meeting of the British Association in 1901 into a paper entitled ' On the necessity of postulating an aether '. He wrote: " there are representatives of every shade of opinion as to the aether, from those who regard it as a wholly unnecessary conception, to those who wish to invest it with every material property that it will support. . . . Those who assert the existence of a medium do so in order to satisfy a craving for the *explanation*. . . . For the *expression* of the laws . . . it is not necessary to introduce any medium ". Such was Hopkinson's estimate of the ethers of the past, but he had developed an ingenious argument which led him to suppose that an ether with specific properties was in fact required to give correct expression to the physical laws which observations on certain double stars, ' spectroscopic binaries ', demand in relation to the propagation of light. In this the passage of time showed him to have been mistaken. It is now clear that Hopkinson's ether was no more necessary than any other, once Einstein's principle had been accepted in its place.

3. NEUTRONS AND RADIATION

There is no evidence to suggest that Ernest Rutherford (1871–1937) ever seriously concerned himself with the merits and demerits of rival theories of the ether, but there is much to show that the idea of a neutron was often in his mind, from an early stage in his career. It emerges first in 1904. Welding together his classic account of the first eight years' work in radioactivity into a connected story filling nearly 400 pages (*Radio-activity*, Camb. Univ. Press, 1904), at a time when one surprising result followed another in disturbing sequence, he reviewed the position that had been reached concerning the nature of the γ-radiation of Villard (1900). " It now remains to consider whether the rays are material in nature or whether they are a type of ether-pulse like Röntgen rays ", he wrote. Either they are " very penetrating Röntgen rays ", or if material, then " they consist of negatively charged particles projected with a velocity very nearly equal to that of light ", or " of uncharged bodies projected with great velocity ". He did not, in the text, decide in favour of one of these possibilities rather than another (only in a footnote, added in proof, recording an experiment of Eve which appeared to " remove the most serious objection which has been urged against the view that the γ rays are in reality X-rays of a very penetrating type "), but at least he noted in passing " a small uncharged particle moving through matter would probably not be absorbed as rapidly as a charged particle of the same mass and velocity ". That conviction—which would not necessarily have been shared by all physicists at that time, for it was based intuitively on a concept of what matter is like at the sub-atomic level—never left him: as we shall see, it was to reappear later with greater effectiveness.

The conclusion that γ-rays are nothing more than ' very penetrating Röntgen rays ' received added support as time went on. In 1912, when he reviewed the position in *Studies in Radioactivity*, W. H. Bragg wrote: " experiment is continually suggesting that X-and γ rays are similar in kind though quantitatively their properties may differ considerably . . . it will be convenient . . . to use the term X-ray to cover the γ ray also, unless, of course, there is reason to make a distinction ". But to Bragg, at least, it did not come naturally at that time to accept the conventional view of the nature of the Röntgen radiation. " I think it is just to say that the aether pulse idea has been for some time unproductive ", he wrote: " It seems to me better to put it aside provisionally . . . To take such a step is no denial of all connection between X-rays and electro-magnetic phenomena: it is but to put down one tool and to take up another better fitted for the moment to the work in hand ".

Over a period of some years Bragg had become increasingly impressed by the inability of the classical pulse theory to explain the production of secondary electrons by X-rays. He was impressed by the fact that, in a radiator at a distance from an x-ray tube, an electron can be liberated with kinetic energy almost equal to the maximum kinetic energy of the electrons falling on the anticathode of the tube, almost instantaneously, whatever the distance. So, forgetting " for the time that idea of keeping touch with electromagnetic theory as we fancy it must be ", Bragg was " led to conceive of a more material x-ray ": " The electron . . . may be imagined as capable of attaching to itself enough positive electricity to neutralise its own charge and of doing this without appreciable addition to its mass. This is the transformation from electron to x-ray: the reversed transformation occurs when the electron puts down its positive charge again. Neither change can occur, except during the passage of the entity through an atom. As an electron, the entity is capable of ionising and so forth. . . . As an x-ray, the entity, being neutral, passes through atoms freely and carries its store of energy from point to point without loss. It is no argument against this view that the positive electron has not yet been isolated, [even a charged particle, if its mass were very much smaller than the mass of the negative electron, might be very difficult to detect] ". Completely frank, Bragg confessed: " When I first put forward the neutral pair theory I was ignorant of the work of Einstein [on the photo-electric effect] . . . I did not think of carrying over the idea to the theory of light . . . ". This was a surprising confession, because Einstein's views had in large measure been anticipated by Thomson, Bragg's early teacher, and, in fact, in some measure Thomson had anticipated Bragg, also. In his Silliman lectures of 1903, Thomson had said: " on the view we have taken of a wave of light the wave itself must have a structure, and the front of the wave, instead of being, as it were, uniformly illuminated, will be represented by a series of bright specks on a dark ground ", and again: " The difficulty in explaining the small ionisation is removed if, instead of supposing the front of the Röntgen ray to be uniform, we suppose that it consists of specks of great intensity separated by considerable intervals where the intensity is very small . . .".

Bragg's confession was indeed surprising, but to labour his original ignorance would be unjust: it is the prerogative of the innovator to disregard the views of others, at least to a point. An unconfessed, but perhaps more surprising, innocence is that in 1912 Bragg should still have thought it possible to maintain that a negative electron, in passing through an atom, might pick up disembodied positive charge. He had been an intimate correspondent of Rutherford during the period in which the nuclear atom was born: on 12 February 1911 he had written to him: " I got your letter of yesterday just now: I have your letter of Thursday also. I am delighted to hear how things are coming out: and I agree with all you say ". Yet a year or so later he did not seem to understand that Rutherford had finally locked away all the positive charge in the atom nucleus. At this distance in time the situation appears truly surprising, but possibly we should not be surprised: it is a not unreasonable opinion that in 1912 no one else fully understood the finality of the revolution that Rutherford had initiated in the previous year, not, it has been opined, omitting Rutherford himself (Andrade 1958).

In support of his neutral doublet hypothesis Bragg noted " [the passage of x-rays] through any substance shows no dependence upon any crystalline structure . . . nor is there any evidence that they can show any diffraction effect; if they could, it would be hard to resist the conclusion that they consisted of some vibrating motion, and the experiment would be looked to for a determination of the wave length ". Had the writing of *Studies in Radioactivity* been delayed more than a few months, this speculation would have been overtaken by the news of the success of the experiments of v. Laue and his pupils. Then the book might have had less coherence, and Bragg's x-ray neutron (though he never called it that) would not have been fitted for the part that he hoped it would play in the development of " some great idea which will reconcile the old antagonism between the corpuscule and the wave ". The words last quoted are Bragg's own concluding words in this most individual of textbooks.

4. Neutrons and nuclei; neglected clues

In the preface to the third supplementary chapter to *Modern Electrical Theory*, published as a separate volume in 1923, Campbell wrote: " almost all the successful ideas in the modern theory of atomic structure have cropped up in the endless speculations that have been made some time before they were finally established. . . . I have followed the principle that, for the purposes of science, the author of an idea is he who first uses it to explain facts. Theories exist to explain facts, and an idea for which there is no experimental evidence is not part of a scientific theory ". This is an austere view of the matter, but, for the writer of a textbook at least, the following of it provides a safe and salutary discipline. The writer of history, on the other hand, may not wish to be bound by it so strictly. He may wish to abate its austerity by broadening his definition of ' fact ', including general qualitative knowledge along with detailed quantitative information, as fact admissible as a basis for theory. If our terms of reference are so extended, we may ascribe to Rutherford's genius not only the concept of the nuclear atom, which he introduced in 1911 to explain the particular fact that the distribution of resultant α-particle scattering in a thin gold foil does not follow the statistical law applicable to a multitude of small deflections, but also the notion of the neutron as a nuclear constituent particle, to which he was led by various considerations of a more general qualitative nature over a period of years, until it appeared, fully fledged, in a Bakerian lecture in the summer of 1920.

In this section, and the two which follow it, we are to consider the related theories of neutrons and nuclei. In so far as they can be separated, we consider the nuclear theories first, for the others developed out of them. At the very beginning it is interesting to turn aside to note the chance that was lost by men of science in the last years of the nineteenth century, the real chance of putting forward a nuclear atom-model, with the mass, at least, concentrated at the centre, in order to explain—or at least to correlate—very definite facts of experience.

Some of these facts were known to Maxwell (1831–1879). In a lecture before the Chemical Society on 18 February 1875, Maxwell said: "And here we are brought face to face with the greatest difficulty which the molecular theory has yet encountered. . . . If we suppose that the molecules are atoms—mere material points, incapable of rotatory energy or internal motion . . . the ratio of the specific heats is 1·66 which is too great for any real gas. But . . . [the molecule] cannot be a mere material point, but a system capable of changing its form. Such a system cannot have less than six variables. This would make the greatest value of the ratio of the specific heats 1.33, which is too small for hydrogen, oxygen, nitrogen, carbonic oxide, nitrous oxide and hydrochloric acid ". In the following year, for the first time, an experimental ratio of 1·66 was found (for mercury vapour), but this isolated case of agreement with the predictions of over-simplified theory did nothing to lessen Maxwell's anxiety: to the end of his life he was disturbed by the fact that for any real gas the ratio was as large as 1·4, let alone 1·66, for it implied above all else that " the many other degrees of freedom of vibration revealed by the spectroscope " are ineffective (Callendar 1902).

Somewhat later, on the basis of a model which no one could possibly have believed in as a replica of reality, Boltzmann (1844–1906) sought to make credible the value of the ratio, 1·40, for the common diatomic gases on the hypothesis that the degree of rotational freedom associated with the principal axis of inertia through the centres of the atoms was ineffective, as, apparently, were all the degrees of ' vibrational ' freedom. There was no conviction, but, seemingly, no alternative. Then, in 1894, Rayleigh and Ramsay discovered argon, and in the following year Ramsay isolated helium. For each of these inert gases the ratio of the specific heats was determined as very close to that for mercury vapour. Reviewing the position in 1900, Rayleigh wrote: " In the case of argon and helium and mercury vapour the ratio of specific heats (1·67) limits the degrees of freedom of each molecule to the three required for translatory motion. The value (1·4) applicable to the principal diatomic gases gives room for the three kinds of translation and for two kinds of rotation. Nothing is left for rotation round the line joining the atoms, nor for relative motion of the atoms in this line. Even if we regard the atoms as mere points, whose rotation means nothing, there must still exist energy of the last mentioned kind, and its amount (according to the law) should not be inferior . . . What would appear to be wanted is some escape from the destructive simplicity of the general conclusion in relation to partition of kinetic energy . . .".

Rayleigh's last remark was oracular, but it was also sterile. There were two difficulties, concerning the rotational and vibrational modes, respectively. Rayleigh merged the two difficulties into one. They had been better treated separately. With regard to the rotational modes, there could be no question but that the significant molecular parameters were the three principal moments of inertia; with regard to the vibrational, the dynamical specification was less obvious. The only plausible interpretation of the facts required, in respect of the rotational modes, that that principal axis of inertia of a diatomic molecule which passes through the centres of the two atoms has different inertial properties from the other two—and properties similar to those of all three principal axes of inertia of an isolated atom. Yet the simple empirical generalisation was never seized upon: "Any principal axis of inertia which passes through the centres of all the atoms in a molecule is practically ineffective in relation to the theorem of equipartition of energy ". To have seized upon this generalisation would have been to endow the centre of an atom with a very special dynamical character—and it could have implied little else than that almost the whole of the mass of the atom is concentrated there. In retrospect we realise that, in the end, both difficulties regarding the specific heats have required the quantum theory for their resolution. That in respect of rotational modes in particular has required the nuclear atom (in relation to its distribution of mass rather than of charge) in addition. In his mathematician's remark: " Even if we regard the atoms as mere points, whose rotation means nothing ", Rayleigh overlooked the physical nucleus. Mere points are not physical entities.

So the slender chance of a nuclear atom model devised for the purpose of ' explaining ' ascertained fact was let slip in the 1890's ; indeed fifty years earlier Faraday had come closer to it than this. Faraday was set wondering by a very simple fact which for him carried profound implication. The compounds of potassium and sodium with elements of smaller atomic weight have densities, in the solid state, many times greater than that of the pure metal in either case. Faraday had identified, we should now say, the large empty space which the valency electron occupies in the atom of an alkali metal—and he did not know what to make of it. In a letter to the editors of the *Philosophical Magazine*, dated 25 January 1844, he wrote: " We may cast into potassium oxygen atom for atom, and then again both oxygen and hydrogen in a twofold number of atoms, and yet, with all these additions, the matter shall become less and less, until it is not two-thirds of its original volume. . . . Surely then, under the suppositions of the atomic theory, the atoms of potassium must be very far apart in the metal . . .". On the assumption of immutable material atoms, this conclusion was clearly inevitable, but it did not satisfy Faraday, and, to escape from it, he was led to adopt—with due philosophic caution—the immaterial atom of Boscovich (1758), and, incidentally, to reject the physical nucleus: " If we must assume at all . . . then the safest course appears to be to assume as little as possible, and in that respect the atoms of Boscovich appear to me to have a great advantage. . . . If, in the ordinary view of atoms, we call the particle of matter away from the powers *a*, and the system of powers in and around it *m*, then in Boscovich's theory *a* disappears, or is a mere mathematical point. . . . Thus, referring back to potassium, . . . surely the *m* is the *matter* of the potassium. . . . To my mind, therefore, the *a* or nucleus vanishes, and the substance consists of the powers or *m* . . . each atom extends, so to say, throughout the whole of the solar system, yet always retaining its own centre of force. This . . . seems to fall in very harmoniously with . . . the old adage, ' matter cannot act where it is not ' ". Faraday identified a large region of empty space within the atom—and came to the conclusion that it was advantageous to consider that all is emptiness, except for the forces: for him the physical nucleus ' vanished '.

At the turn of the century Thomson and Lenard were again finding emptiness within the atom, in another context altogether. In the letter just quoted, Faraday had remarked: " Light and electricity are two great and searching investigators of the molecular structure of bodies . . .". Electricity, in the form of the cathode rays, had done the searching in this later instance. On 30 April 1897 Thomson delivered an address before the Royal Institution in London. More than any other single communication, this address may be regarded as announcing the negative electron to the world. Thomson discussed Lenard's experiments on the absorption of the cathode rays outside the discharge tube. This is how he pictured the phenomenon: " the atoms of the ordinary elements are made up of corpuscules and holes*, the holes being predominant . . . at the cathode some of the molecules of the gas get split up into these corpuscules, and . . . these, charged with negative electricity and moving at a high velocity, form the cathode rays . . . the things these corpuscules strike against are other corpuscules, and not against the molecules as a whole; they are . . . able to thread their way between the interstices in the molecule ".

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* Not Diracian ' holes '!

In 1903 Lenard published an account of a much more exhaustive study of the absorption of the cathode rays. And he produced a much more detailed picture of the process. He referred to " the old hypothesis . . . that the different atoms of all [types of] matter are built up of constituent particles of the same kind but in different number " and wrote: " To be able to refer unambiguously to these constituent particles . . . I call them dynamids. Each atom is then constituted of a number of dynamids proportional to its weight . . . to each dynamid a given absorption cross-section is to be ascribed . . . the dynamids are to be considered as electric force fields . . . if in addition one assigns an impenetrable centre then its cross-section . . . will be referred to as the true cross-section of the dynamid . . . the true radius of the dynamid must be less than $0{\cdot}3 \times 10^{-10}$ mm ".

Granted Lenard's assumptions, this was a conservative estimate, based on the assumption that the atom of hydrogen contained a single dynamid only (the radius would have been smaller still if more had been assumed)—and he proceeded to conclude that not more than one part in 10^9 of the volume of solid matter is other than empty, ' as the heavens are empty ', even though it were solid platinum. This numerical assessment went a long way beyond Thomson's ' predominance of holes ', but Lenard attempted to abstract too much from his model: the basic simplicity of a common structural unit was lost when he was led to assume ' that the dynamids of different species of atom possess small characteristic differences ' and, imagining ' the simplest constitution of a dynamid would be an electric doublet of given moment ', to postulate that these characteristic differences might be differences in dipole moment. Then, to preserve the macroscopic bulkiness and opacity of solid matter, he endowed his dynamids with motion within the atom, and, to ensure the stability of their force-fields, with inner rotation also. What had begun as a significant evaluation of the emptiness of atoms degenerated into a wholly extravagant mental picture of their constitution: it is hardly surprising that it attracted little further interest. For our purpose, however, Lenard's dynamids were neutrons, and his ' total dynamid volume ' set an upper limit to the volume which might belong to the nucleus, if someone, at a later date, were to picture a nuclear atom.

The first person to make such a picture did so speculatively, and ineffectively, and entirely without reference to Lenard. Late in 1903 Nagaoka communicated a paper to the *Philosophical Magazine* entitled ' Kinetics of a system of particles illustrating the line and the band spectrum and the phenomena of radioactivity '. It was a mathematical exercise based on Maxwell's classical discussion, in his Adams prize essay of 1857, of the constitution and stability of the ' rings ' of the planet Saturn. Nagaoka wrote: " The system, which I am going to discuss, consists of a large number of particles of equal mass arranged in a circle at equal angular intervals and repelling each other with forces inversely proportional to the square of the distance; at the centre of the circle, place a particle of large mass attracting the other particles with the same law of force . . . The system differs from the Saturnian system considered by Maxwell in having repelling particles instead of attracting satellites. The present case will evidently be *approximately* realised if we replace these satellites by negative electrons and the attracting centre by a positively charged particle. . . .". This was the Saturnian (nuclear) atom, the first suggestion of which Nagaoka attributed to Lodge, but it was quickly forgotten, for it explained nothing.

About the time that Nagaoka's paper was published Thomson wrote to Lodge: " With regard to positive electrification. . . . I have, however, always tried to keep the physical conception of the positive electricity in the background because I have always had hopes (not yet realised) of being able to do without positive electrification as a separate entity, and to replace it by some property of the corpuscules. . . . all the positive electricity does, on the corpuscular theory, is to provide an attractive force to keep the corpuscules together, while all the observable properties of the atom are determined by the corpuscules. . . .". Two years later, however, Thomson himself had recognised the existence of ' observable properties ' which required that this otherwise wholly admirable interim view should be revised. In a masterly paper, ' On the number of corpuscules in an atom ', he wrote: " I consider in this paper three methods of determining the number of corpuscules in an atom . . . all of which lead to the conclusion that this number is of the same order as the atomic weight of the substance . . . from the first method we conclude that the mass of the carrier of unit positive charge is large compared with that of the carrier of unit negative charge. If we suppose the whole mass of an atom to be that of its charged parts, e/m for the positive unit charge would be of the order of 10^4 ".

The ' first method ' referred to by Thomson depended on a theoretical expression for the optical dispersion of a gas: Thomson did not, however, recall the fact that already in 1900 Larmor had come to a similar conclusion regarding the mass of the positive particle, qualitatively, in his discussion of the Zeeman effect. " If we regard the molecule as constituted of a system of ions, of various effective masses denoted by m and electric charges denoted by e . . .", Larmor wrote, " suppose the charges to be the same for all the ions, and the effective masses of the positive ones to be large compared with those of the negative ones which must be themselves equal. . . . If the positive ions are not more massive in this manner, this simple representation of the effect . . . will not hold good ". Larmor's argument had been at least as direct as Thomson's own; in any case their conclusions were essentially the same.

In 1906, then, Thomson abandoned his attempt to do without positive electrification as a separate entity. Still requiring a convenient model, however, he reverted to the notion of a uniform sphere of positive charge, occupying the whole volume of the atom and with the electrons embedded in it. But now he had estimated the number of electrons involved: he could no longer hold this view literally, therefore, and at the same time imagine, as we have seen that he did in 1903 (and again in 1907!) that the whole mass of the atom is bound etherial mass. His biographer wrote of him: " He was very prolific in explanations of the difficulty which immediately confronted him, but it was not perhaps in accordance with his habit of mind to work round the horizon to see what other difficulties he might be introducing . . . he usually preferred to dwell on what a theory *would* explain than on what it would not ". It was so in this instance, and, in relation to the positive charge in particular, what he himself wrote in 1903 remained true of all variants of his thought on this subject: " although the model of the atom to which we are led by these considerations is very crude and imperfect, it may perhaps be of service. . . .".

The atom model of Thomson was, in fact, of service for another five years: it was the only simple model which, on the basis of classical theory, guaranteed stability for the system; its properties were calculable—and they provided more than a hint of an explanation of the regularities which are summarised in the periodic table of the chemist. It might have served longer had not Rutherford been moved, by some strange prompting of insight, early in 1909, to let ' young Marsden ', under Geiger's direction, try his hand, as an introduction to more serious research, on a ' fool experiment '.

To be concluded

References will be given at the end of the article.

Cosmic radiation : The primary particles

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Summary

Observations of primary cosmic-ray particles entering the upper atmosphere show that the solar system is situated in an isotropic flow of protons and of heavier nuclei, and that these particles carry energy which can vary from small values to values so great that we cannot at present define the upper limit of energy carried by a single particle.

These primary particles are constrained to diffuse through the galaxy as if in a reservoir and must initially have been injected into the reservoir from specific sources. In many ways the turbulent clouds which remain at the site of eruption of supernovae meet the requirements of such sources, and this property can be related to the identification of these objects as sources of strong radio-emission or radio-stars.

1. Present divisions in cosmic-ray physics

It is now about sixty years since the possibility of an ionizing radiation entering the atmosphere from without was first mentioned in scientific papers. Nearly half of the time which has elapsed since these early speculations was needed to produce general conviction in the reality of a ‘ cosmic radiation ’, and systematic studies of the phenomena involved have gone on now for only about thirty years. But the work of these years has been fruitful beyond any reasonable expectation, and has contributed notably both to the physics of the very small—in the behaviour of atomic nuclei in interaction and in conditions of great excitation—and to the physics of the very large—concerning the structure of our solar system and, on yet another scale, of the galaxy itself.

Success over so wide a field has of necessity led to specialization, so complex and so interesting are the problems posed under any one of several headings. At the present time we can distinguish three main headings; in the next section the salient matters of interest under each are summarized, and then in the second part of the article a progress report is given on one of these, the problem of primary cosmic rays as a galactic phenomenon.

In the first place, the interactions of primary cosmic-ray particles reaching the earth, entering the atmosphere and making nuclear collisions in it, continue to be of great interest. For the past twenty years observations of such interactions have led work in nuclear physics at very high energies, and repeatedly phenomena first observed in the random natural flux of cosmic-ray particles have, with the development of larger (and more costly) accelerators, come within the range of controlled experiment under laboratory conditions. This process has reached to energies of bombarding particles of the order of 10^{10} ev; now, while enough is already known about the behaviour of cosmic-ray primaries of very many times this energy to excite great interest, the effort and resources needed to accelerate particles artificially to comparable energies are becoming so large that the transition to laboratory conditions is not likely to follow anything like so quickly as it has done even in the recent past. Indeed, for the highest energies for which significant information is now becoming available, artificial beams of particles do not seem probable at any time in the forseeable future. At these energies, therefore, studies of the interactions of cosmic-ray particles are not merely a preliminary survey guiding machine development, and attention is increasingly being directed to improvements of technique and of interpretation and to wider and longer-term plans for work.

Secondly, and very far removed from these problems about nuclear interactions, there are problems which have excited imagination since the extra-terrestrial character of cosmic radiation was first established. How, where and under what conditions have the primary cosmic-ray particles been brought to the very high energies with which they reach the earth, energies differing by many orders of magnitude from, and apparently quite out of equilibrium with, the energy content of practically all other constituents of the known universe? How have these particles moved since this initial acceleration, over what space are they distributed and what ultimately happens to the great majority which, we must suppose, do not ever encounter any condensed massive object such as our earth, with the atomic nuclei of which collisions may take place?

In the early days of the subject, these particular problems were thought of in a quite simple way: it seemed likely that the primary particles represented streams flowing away in direct paths from identifiable sources in some part of the stellar system, and evidence of the directions of such sources was very extensively sought. Further thought, however, and more complete knowledge about the nature and energy distribution of the primary particles which actually reach the earth, and about the space in which they move, have convinced everybody that the problems are far more complex. They are, moreover, closely related with other problems of astrophysics and cosmology, and are in fact among the striking features of our galaxy which, with others, enable us to visualize its structure and behaviour.

Thirdly, a group of problems which have come very much to the fore in recent years, and particularly during the term of the International Geophysical Year, fall in many respects between those outlined in the last three paragraphs. These concern the influence of the solar system on the primary particles which approach it (including all those which finally impinge upon the earth), and arise because of the surprisingly complicated electromagnetic fields which exist within the solar system. These fields deflect the trajectories of cosmic-ray particles which pass through them, and under some conditions may accelerate or decelerate the particles. The important fields in this respect seem to be (i) the more or less static magnetic field of the earth itself, and (ii) the very changeable regions of field carried out from the sun in streams of ionized gas far into the solar system, even to beyond the earth. These streams are of differing kinds, and the effects which they produce upon cosmic-ray particles approaching the earth depend upon the extent to which the particle trajectory passes through the stream, and the direction in which it does so. The most notable feature, however, of these solar streams is the way in which the intensity and frequency of emission waxes and wanes with a period of about eleven years: the corresponding variation of sunspot eruption, a closely related phenomenon, is well-known, with times when spots are few in number and small, as they were in 1954, and then times of many and large spots as in the latter part of 1957 and in 1958.

While as phases in the life-history of a single primary cosmic-ray particle the three stages which have been discussed above are quite distinguishable, they are not at all separable when it comes to observation and experiment. It is only possible to deduce the motion of primary particles before they approach the solar system by recognizing and allowing for the effects of their passage through it, and again it is only possible to recognize the most energetic primary particles which reach the earth by the very large ' showers ' of particles which arise in a succession of stages after these primaries have made initial collisions with nuclei of the atmosphere.

In a short article it is necessary to accept the separation of interest which has been defined in the last few paragraphs, and in what follows our subject will be limited to features of the primary radiation, for which the earth is a probe at which measurements may be made and from which we may speculate about its problems.

2. OBSERVATIONS ON THE PRIMARY PARTICLES

It is possible at any point on the earth, at altitudes so high that the effect of absorption in the atmosphere and of the transformations which follow a nuclear encounter by the primary in the atmosphere are relatively unimportant, to make observations of the direction of arrival of the primary particles, their nature and their energy. In many ways it would be well to get right outside the atmosphere for this purpose; but up to the present satellites and space-probing vehicles have only been able to carry and report observations with very simple pieces of apparatus. By far the greatest part of our direct information about the primary radiation is obtained at balloon altitudes, that is to say at heights of about 100 000 feet. The balloons used for this purpose are large, unsealed bags of thin plastic, and the design of the container and operational experience in preparation and in launching have reached a stage at which the loaded balloon can be caused to float at a steady height for several hours.

The nature of the primary particles is most effectively studied by loading such balloons with stacks of nuclear emulsion sheets, material of the same nature as that used in photography, but modified for the particular purpose of registering the passage of charged particles through the emulsion rather than the incidence of light upon it. The so-called ' nuclear research emulsion ' contains a much higher proportion of silver bromide than does a normal photographic emulsion, and it is made in much thicker layers since otherwise only extremely few particle trajectories would lie nearly enough in the plane of the emulsion to yield useful lengths of track. It cannot be made in thick blocks, as would at first sight seem desirable, because of difficulties in securing uniform development and in avoiding distortions during development and fixing of the soft damp emulsion; also, of course, the tracks are extremely fine and have to be found and measured under a high-power microscope which will have a very limited working distance.

The tracks which are attributed to primary particles are those of atomic nuclei completely freed from extra-nuclear electrons and extending from protons, the lightest possible nuclei, up to nuclei about as heavy as that of iron. The tracks are distinctive in that protons are always distinguishable from the heavier nuclei while, provided that at least a millimetre of track of a heavier nucleus is available, the uncertainty in atomic number of the particle which has produced it will only be one or two units. In exposures made at balloon altitudes it cannot be assumed that all tracks of protons and of heavier nuclei are those of primary particles; some at least of the particles of low charge will be fragments of heavier primary nuclei which have already undergone a first, disruptive, interaction in the residual layer of air above the level of exposure. The correction of the observed relative frequency of nuclei as a function of the atomic number, Z, for such fragmentation is the greatest uncertainty in our knowledge of the composition by charge of the primary radiation.

Nuclei	Primary cosmic radiation abundance	Universal abundance
Protons	100,000	100,000
α-particles ($Z=2$)	10,000	7,700
Li, Be, B ($Z=3$ to 5)	$\sim$50 (?)	$<10^{-3}$
C, N, O, F ($Z=6$ to 9)	520	80
$Z\geqslant10$	160	30
Fe ($Z=26$)	30	1·5
$Z>30$	<1	10^{-3}

Relative abundance of atomic nuclei in primary cosmic radiation, and, for comparison, the ‘ universal abundance ’ of these nuclei.

The table shows the relative abundance of the different main groupings of nuclei which are in general use as deduced from emulsion observations for the primary radiation. For the purpose of a comparison which is made later, the table also shows the estimated ‘ universal abundance ’ of nuclei collected in the same groupings. These figures, which are based on work by Suess and Urey, command general acceptance, but they refer to a process of estimation altogether more difficult than that involved in determining the charge composition of cosmic-ray primaries, and cannot be expected to be as certain. In spite of this reservation, however, the form of difference between the data of the second and third columns of the table must be regarded as very strongly based. The figure given, also, for the cosmic-ray abundance of the nuclei of lithium, beryllium and boron has an exceptional degree of uncertainty, which arises from the rather low abundance of these primaries, from the relatively frequent production of them in fragmentation of heavier nuclei and from certain difficulties of identification which are less severe both for lighter and for heavier primaries.

2.1. *Electrons as primary cosmic-ray particles*

Nothing has been said above about the presence of electrons in the primary cosmic radiation reaching the earth, and indeed up to now no electron component has been detected either by examination of exposed emulsions or by other experiments where, for example, a cloud chamber, in which the characteristic behaviour of electrons passing through absorbing plates might be observed, is flown to balloon altitudes. Moreover, a secondary electronic component, following quickly after the initial nuclear collisions of protons and heavier particles of the main primary radiation, develops to considerable intensity even at balloon altitudes, and so the presence or absence of small numbers of primary electrons is not easily established. It is fairly certain that, energy for energy, electrons are not present in numbers greater than about 1% of that of protons, and they may well be much less abundant. The evidence is not that electrons are entirely absent, and it will be seen later that data from other sources seem to require a substantial electron population at cosmic-ray energies which, however, is still well below the limit just stated.

2.2. *Distribution of energy among cosmic-ray primaries*

The determination of the distribution of energy among cosmic-ray primaries is a much more complicated problem than that of the relative abundance of nuclei of different charge. The general situation may be quite simply stated: confining attention first to protons, at low energies, up to perhaps 10^8 ev or even 10^9 ev, the approach of primaries to the earth is very seriously impeded by quite local phenomena taking place within the solar system. Passing to still higher energies, where local solar effects become less important, the number of primaries with given energy varies smoothly with energy, the higher the energy, the fewer the number of primaries found to have that energy.

Such an energy distribution (or energy spectrum) among the primary protons can be represented by an expression of the form:

$$N\ (E)\ dE = E^{-\gamma}\ dE,$$

where N is the number of primary protons of energy between E and $E+dE$ per unit area, per unit time and per unit solid angle. Over short ranges of energy the index γ can be regarded as a constant, but to represent the whole known primary spectrum γ will be a slowly varying function of energy. The spectrum can no doubt be extended to the lower energies where our experimental information is defective; as far as observations are concerned it extends indefinitely towards higher energies, a slowly changing index γ still applying when, at exceedingly great energies, the flux of particles is measured in particles per square kilometre per month rather than in particles per square centimetre per second. This limit of observation, arising because of the scarcity of particles, lies at within a factor of ten either way of 10^{18} ev, and so the experimentally known part of the primary proton energy spectrum extends over a range of 10^{10}.

It is not surprising that a single method of measurement of energy is not suitable over the whole of this range, and, still considering the proton primaries, we distinguish three different lines of evidence which are applicable to successively higher ranges of energy. For energies up to about $1{\cdot}5 \times 10^{10}$ ev advantage is taken of the operation of the magnetic field of the earth as an energy analyser; at the magnetic equator protons are able to reach the earth in the vertical direction provided that their energy is greater than this value, as we move from equator to pole the general situation is similar in that protons of all energies from a certain limiting value upwards can reach the earth from the zenith, but the limiting value falls steadily in a calculable way and would reach zero at the pole. The different numbers of protons observed in the vertical direction at the top of the atmosphere at different latitudes can thus be directly related to the energy spectrum up to the value $1{\cdot}5 \times 10^{10}$ ev. At high latitudes the limiting energy falls within the region subject to strong local disturbing effects, but from energy about 10^9 ev upwards this energy region is probably the best studied of any.

For energies greater than $1{\cdot}5 \times 10^{10}$ ev this approach fails. However, primaries are still abundant enough to pass in considerable numbers through suitable emulsion stacks, and it becomes necessary to estimate the energy of primaries which undergo nuclear interactions in the body of the stack from the results of the interactions, from the angular spread, and from the nature and the energy of the secondary particles. There is immediately some degree of uncertainty because the energies concerned are all beyond those for which direct experience is available with laboratory sources, but energy estimates of this kind allow us to extend the photon spectrum with fair confidence up to about 10^{13} ev. At still higher primary energies the number of protons falling on a small piece of apparatus flown for a few hours on a balloon becomes negligibly small, and the estimates of the extension of the primary spectrum here are based on observations of the full secondary development which arises in the atmosphere from each of these very energetic particles, the so-called ‘ extensive air showers ’. The general mechanism of the formation of these showers has been known for a long time, and there is little doubt that the greater part of the energy of the primary particle is transferred to a stream consisting of large numbers of electrons and photons for which both the energy distribution and the spread sideways from the trajectory of the primary particle can be predicted. To a first rough approximation, the number of electrons when the shower has grown to its maximum size may be taken as proportional to the energy of the primary particle, and for the large showers the number of electrons in the shower at sea-level is sufficiently close to that at maximum growth. The importance of the showers for deducing the energy spectrum of primary particles at the very high energies, for which the number of particles is small, lies in the fact that the spread of electrons away from the line of the original trajectory is considerable, and they may be detected over an area of up to a square kilometre. The total number of electrons is estimated by a sampling method, using detectors set out in a lattice-like array over such an area; the array as a whole is then equally sensitive to primaries the axes of which would have struck anywhere within this area, and an estimate of the energy of primaries can then be made at the extremely high energies for which the primary flux may only be a few particles per square kilometre per month.

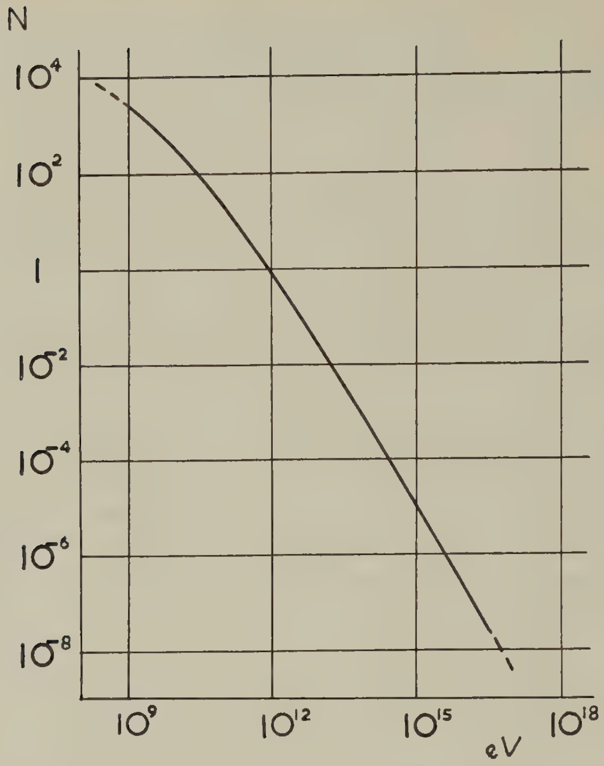

Fig. 1. Integral energy spectrum of primary cosmic-ray protons, showing the number $N(E)$, of particles of energy greater than E per square metre per second per unit solid angle. (A memorable figure is that particles of energy greater than one erg occur at a rate of about one per square metre per second per steradian; 1 ev$=1 \cdot 6 \times 10^{-12}$ erg.)

The broad correctness of this procedure and of the deductions from it is not in doubt. It is virtually certain that the energies of the primaries from which the largest known showers are derived do lie within the range 10^{17} ev$<E<10^{19}$ ev, but for several reasons this rough approximation is not all that is wanted. In particular, the smooth continuation of the primary spectrum up to these extremely high energies is a matter, as we shall see, of great interest and even of difficulty. The question has therefore become important as to whether the approximation in which a primary of given energy always gives rise to a shower of the same given size is at all adequate. If it is not, then account must be taken of the fluctuations of development which may lead to differences, perhaps very large differences, in size of shower from primaries of a single energy. This whole matter occupied a prominent place in the discussions of the International Cosmic Ray Conference held in July 1959 at Moscow. At the moment a good deal of caution is necessary in drawing any conclusions from the highest measurements of the primary spectrum. Figure 1 shows the primary proton spectrum as it is now understood, it is in the form of a double logarithmic plot and shows an absolute rate of primary particles.

2.3. *Energy spectra of primaries other than protons*

The last section was concerned with the primary proton spectrum; the spectra of α-particles and of heavier nuclei are less easily studied because the primaries are less abundant, and, for what is usually described as the heavy (H–) group of nuclei ($Z \geqslant 10$), very much less abundant. The first two ranges of energy yield some data, for the emulsion tracks of different kinds of nuclei can be distinguished, but the contribution of heavy nuclei to the high energy region, where primary energy is deduced from the size of extensive showers, is quite unknown. The value used in fig. 1 is in fact that due to all primaries, although there is little doubt that the protons predominate. At the moment it appears that the energy spectra of all primary nuclei are very similar providing that they are expressed in energy per nucleon (that is to say, energy per particle in the complex nuclei each compounded of, say, Z protons and N neutrons). If anything, the spectra of the heavier primaries fall off rather more quickly than does the spectrum of primary protons, so that the proportion of protons probably increases slowly to the higher energies. It is the approximately similar form of spectra for all primaries which allowed us to discuss first and separately the charge distribution of primaries before any consideration of the energy spectra.

2.4. *Directional properties of the primary radiation*

We have now to consider what can be found out about the directions of motion of the primaries. This presents immediate difficulties because the majority of particles which reach the earth do so along trajectories which are deflected substantially in the magnetic field of the earth, so that primaries of different energy arriving in a given direction were coming from quite different directions when they were still far from the earth.

However, in the course of its rotation, the earth causes apparatus at a given point to scan at a known and steady rate a succession of directions in space, and, unless there is a quite improbable cancelling out of the effects of primaries of different energy, any departure from isotropy in the streaming of primary particles near to the earth but before they enter its deflecting field will appear as a periodic variation. If the anisotropy is local to the solar system, related for example to the magnetic clouds emitted from the sun, it will lead to variations with a period of one solar day: such an effect indeed takes place and is of great interest, but falls outside our immediate purpose. If, on the other hand, the anisotropy is related not directly to the solar system but rather to the region of space through which the solar system is moving, defined in relation to its position in the galaxy, the variation will take place with a period of one sidereal day, the time of rotation of the earth relative to the fixed stars and not relative to the earth–sun line.

Many experiments have been carried out in an effort to detect such a sidereal effect, some seeking a variation of the general cosmic-ray intensity at the observing station, some analysing changes in a directional intensity, for example in the vertical direction, and some again looking for a sidereal diurnal variation in the primaries of large atmospheric showers, particles of great energy which can only be deflected to a negligible degree by local fields and disturbances. While some trace of an effect has many times been reported, in no instance have extended experiments confirmed the original indications. This applies to measurements of all kinds, and at the present it is accepted that any sidereal effect that there may be is very small, and that up to now no such effect has been established.

At a single point on the earth this null result might well be a peculiarity of the particular scan of space made by apparatus at that point in each sidereal day, but it seems quite beyond any reasonable degree of chance that such an untypical result would be repeated at stations widely spread all over the earth, and would be alike both for the effects of particles which we know are deflected and spread out in energy and for those of particles of great energy which are hardly deflected at all. We are driven to the conclusion that cosmic-ray primaries over the whole range of energies which we have investigated are moving very nearly isotropically in the region of the galaxy in which the solar system lies.

A sidereal day differs from a solar day by only one part in three hundred and sixty-five, and it is unfortunate that there is not space to discuss the interesting problems of analysis which arise in separating variations which are characterized by these very nearly equal times.

3. INTERPRETATION OF THE DATA ON PRIMARY COSMIC RADIATION

We may summarize the experimental material which has been assembled in the first part of this article as follows:

(i) the number of electrons is only a small fraction of the number of nucleonic particles in the primary radiation,

(ii) there is a greater proportion of heavy nuclei in the radiation than is estimated for the universe at large (see table),

(iii) the energy spectra of all types of primary appear to be similar, these extend smoothly at least to energies of the order 10^{17} ev and may be represented by a power law of slowly varying index, γ, which is of the order 2·8,

(iv) near but outside the solar system cosmic-ray primaries of all energies move almost isotropically.

By a direct derivation from the data of (iii) above we can add a useful fifth point:

(v) the energy density carried by cosmic-ray primaries in the neighbourhood of the solar system is about 10^{-12} erg cm^{-3}.

3.1. *Hypothesis of extra-galactic origin*

What is at first sight the most likely explanation of the observed isotropy of the radiation, that sources of cosmic rays are equally distributed in all directions from the earth, suggests strongly that cosmic rays are extra-galactic in origin, for the sun is so far removed from the central region of the galaxy that it is difficult to imagine any galactic system of sources which would appear equally distributed around it. There are, however, two reasons which make this suggestion unattractive, although neither is really decisive.

Firstly, the energy density, (v) above, is very similar to that of starlight and by implication not out of line with other modes of energy dissipation in the galaxy. But this similarity would fail completely if we are comparing cosmic radiation, assumed not of galactic origin, with starlight, known to be almost entirely of galactic origin, *at points within the galaxy*. The aggregate energy carried by cosmic-ray particles would be enormously increased; and since, if our own galaxy were not effective as a source as viewed from within its own boundaries, it would be surprising if other galaxies were to prove to be intense sources, the problem of origin would become even more stringent than will be shown necessary.

Secondly, if cosmic-ray particles were moving isotropically in the space around our galaxy, and penetrating it freely, it was pointed out as long ago as 1935 by Compton and Getting that the rotation of the galaxy, which gives to the peripheral region in which the solar system lies a velocity relative to the distant galaxies of the order 300 km sec^{-1}, would give rise because of this velocity to a measureable anisotropy. Although this has been estimated to be only about 0·1% to 0·2%, the evidence against its existence is fairly strong.

It will be clear from these last paragraphs that any advantage which attaches to the assumption of an extra-galactic origin of cosmic rays will be lost if the galaxy does not behave as if it were completely transparent to the particles, because the only strong argument for extra-galactic origin is based upon isotropy. It is necessary to ask whether one would expect the galaxy to act as though it were quite transparent to fast charged particles in this way. It has been observed already that because of solar activity the space immediately round the earth does not have this property. Many of the phenomena illustrating this fact are rather complex, but it is worth recalling here one famous and straightforward example. At 4.00 a.m. on 23 February 1956, when conditions around the earth were certainly fairly disturbed, a very exceptional flare on the sun led to the emission of particles, which were quite indistinguishable from low-energy cosmic-ray primaries, for a time of getting on for one hour. During this time these particles, which were detected as an increase of cosmic-ray intensity, reached the earth preferentially in particular regions determined by the direction of the source and, of course, by the deflection of the particles in the terrestrial field. Then emission ceased, but the particles did not quickly pass away from the neighbourhood of the earth. The concentration of the extra particles in preferred regions related to the direction to the sun came to an end, and they were observed as though they were moving isotropically in a region which contained the earth. The intensity of the particles gradually diminished, but it was 24 hours before all the additional particles emitted from the flare had been dispersed. It is evident that on this occasion not only were the trajectories of these particles deflected but the deflections were so large and so frequent that the particles could only slowly diffuse out of the region into which they had been injected, and that in that region they then appeared to be moving isotropically although they came from a single and not very distant source.

There is little doubt that in such events the deflections arise from the passage of the particle trajectory through regions of relatively strong magnetic field. Clouds of ionized gas are ejected from regions of the sun near sunspots where intense magnetic fields are known to exist. On the scale of the phenomenon, the separation of the ionized cloud and the associated magnetic field happens negligibly slowly, but the motion of the whole is determined by a complex interrelation of hydrodynamical and magnetic forces, and the cloud may be expected to retain its identity to an extent beyond what would happen if there were not the additional magnetic constraint on the motion of the ionized particles of the cloud. The ' clouds ' are, of course, of a density which would be described as an extreme high vacuum in the laboratory, and it is with the entrained magnetic fields and not with the matter of the cloud that cosmic-ray particles interact.

3.2. *Motion of cosmic-ray particles within the galaxy*

Purely astronomical observations provide clear evidence that well-defined, coherent gas clouds move in interstellar space. These were first detected by narrow absorption lines on the spectra of stars, and examples were found in which the light from the star passed through two or more clouds with discretely different velocities in the line of sight, giving rise to two or more narrow absorption lines separated by the Doppler effect. The cloud velocities demonstrated in this way were of the order 0·01 km $\sec^{-1}$. Optical measurements provide little evidence that these clouds hold entrained magnetic fields. Observations of the polarization of the light of distant stars have been interpreted in terms of the orientation of dust particles in the path of the light. It is true that dust of suitable composition would take some degree of orientation in a magnetic field, and that if the direction of field was the same over a sufficient length of path polarization would be observed, but it is by no means certain that other explanations might not be given.

Within the last few years, however, very convincing evidence for the widespread existence of magnetic fields through the whole galaxy has come from the work of radioastronomers. The background radio-emission of the galaxy exhibits a strong non-thermal component (a component for which the relation between intensity and frequency is widely different from that required for equilibrium temperature radiation), and this, together with similar emission from the neighbouring Andromeda galaxy, is attributed to the radiation of accelerated electrons, the acceleration taking place in interstellar magnetic fields. The measurements provide material of great interest: they indicate that the galaxy described by this radiation is more nearly spherical than is the plate-like visible galaxy. Reasonable postulates about the magnetic fields yield plausible values of the electron population in accord with (i) on page 212 and the frequency dependence of the radiation may be related to the energy spectrum of the electrons. In bringing into evidence this information about the electron population, which is negligibly small in the primary cosmic-ray flux, we have to notice that the radiation of energy by a fast-moving charged particle of mass m under a given accelerating force is proportional to m^{-2} and that detectable radiation from electrons is to be expected under conditions when that from very much more numerous protons is not observed.

It is not seriously in doubt that the interstellar gas clouds are ionized and therefore conducting, and that they must entrain the magnetic conditions of their origin. It is plausible to regard them at least as magnetically different from the surrounding space. Under these conditions it is to be expected that the space of the galaxy constitutes a diffusing medium for the motion of cosmic-ray particles, comparable on its own scale with that formed from time to time near the earth, and that the isotropy of cosmic rays ((iv) on page 212) is an isotropy of diffusion rather than of origin.

3.3. *Acceleration of particles diffusing among magnetic clouds*

Astronomical data indicate that the gas clouds occupy a relatively small fraction of galactic space, and that cosmic-ray particles moving among them may be considered to have free paths between a succession of collisions with the clouds. The detailed paths followed by charged particles coming into a region of changing magnetic field are infinite in variety, and we shall do no more here than indicate, in fig. 2, two very different examples of the reflection of particles

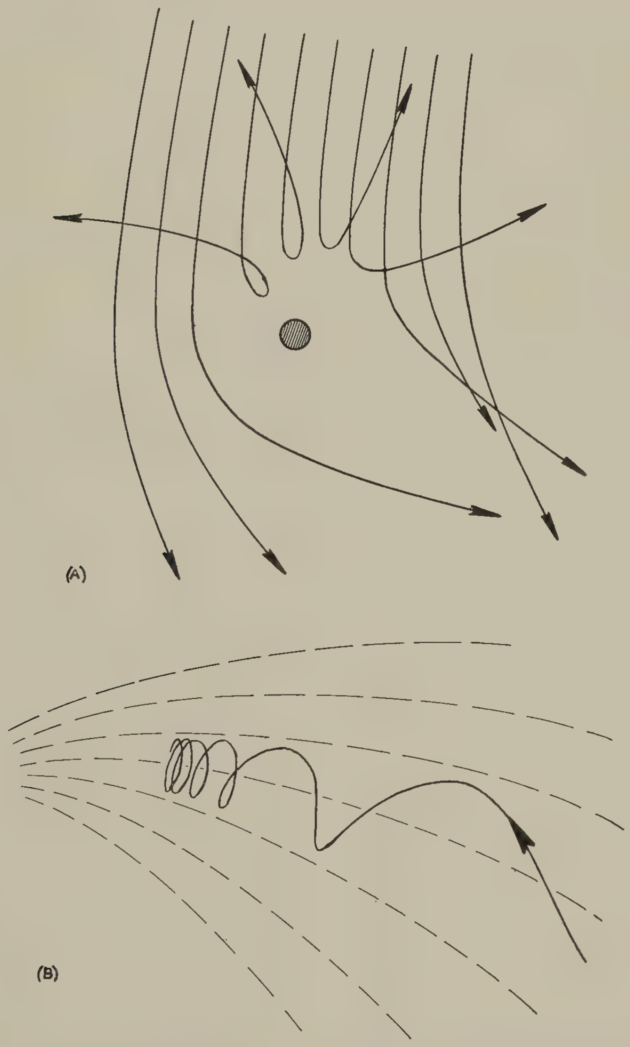

Fig. 2. Examples of the scattering of charged particles in magnetic fields: (A) scattering in the equatorial plane of a magnetic dipole, (B) trajectory of a particle spiralling a line of force and reflected in a region of increasing field strength; as the particle moves to a region of stronger field the pitch of the spiral decreases and reflection takes place when the pitch becomes zero; only the ‘ inward ’ half of the trajectory is shown.

in such regions. Under these conditions, a most striking possibility was first pointed out by Fermi in 1949. This was, that in a statistical average, particles would tend to gain energy over many such collisions, because collisions in which particle and cloud were moving towards each other would happen more frequently than those in which the particle was moving so as to overtake a receding cloud. Collisions in which the particle gained energy would therefore be more frequent than those in which it lost energy. What has just been said concerns the mechanism of energy gain. Looking at the problem more widely and bearing in mind that the clouds must behave for this purpose as massive bodies, the Fermi mechanism essentially proposes that the mixture of magnetic clouds and of cosmic-ray primaries which occupies a great deal of galactic space will tend towards equipartition of energy between the two types of entity concerned, even though the mass difference between the two types is fantastically large.

The rate of gain of energy by the Fermi acceleration is

$$dE/dt = \alpha E = \frac{u^2}{c^2\tau}E = \frac{u^2 v}{c^2 l}E \doteq \frac{u^2}{cl}E,$$

where u, v and c are the velocities of the gas cloud, the particle and of light, τ is the mean time between collisions and l the mean free path. However, particles will not be accelerated indefinitely; either they will escape from the region of the moving clouds or else they will make catastrophic nuclear collisions with the interstellar gas or with dust. We write for the mean life of the particle under conditions of acceleration T. In the steady-state condition, when the whole has been in existence very much longer than T, and assuming a steady injection of particles of low energy into the space, the energy spectrum of particles can be determined:

$$N(E)\,dE = K\,.\,E^{-[1+(1/\alpha T)]}\,dE,$$

where $N(E)$ is the number of particles in unit energy range at energy E in, for example, unit volume at any instant, or incident on unit area in unit time.

We shall not consider here the question of the initial energy with which particles must be injected: the problem of injection is likely to prove only of minor importance.

The Fermi mechanism was put forward in the first place as a method by which the acceleration of cosmic-ray primaries takes place during diffusion in the main space of the galaxy, but there are serious difficulties in this proposal. In the observed energy spectrum of primaries at the earth:

$$N(E)\,dE = K\,.\,E^{-\gamma}\,dE,$$

the index γ lies between 2 and 3 for all energies and for all types of primary, and therefore:

$$\alpha T \sim 1,$$

α being not related in any way to the properties of the diffusing particles. Accordingly, T also must be independent of the nature of the primary and therefore cannot be determined by nuclear interaction in the matter traversed. (The lifetime T if determined by collision would be expected to be ten times shorter for oxygen or carbon nuclei than for protons.) On the other hand, if T is considered to arise from diffusion out of the galaxy, further difficulties arise. If the best values of u and l from other sources are placed in the expression for dE/dt, and T is estimated using the same value of l for a time of diffusion to the boundary of the galaxy from the centre, αT proves to be orders of magnitude too small; if l, the most uncertain factor, is changed to alter α it causes T to change in a compensating manner and no reasonable adjustment of the constants gives much advantage.

We conclude, therefore, that as far as available data go, the amount of acceleration undergone by cosmic-ray primaries during diffusion in the galaxy is probably slight; the primaries must enter the galactic reservoir with substantially the spectrum which is measured at the earth.

The Fermi process is fundamentally attractive, because it does not depend on a special situation but can take place wherever moving clouds carry entrained magnetic fields. This distinguishes it from inductive processes, analogous to those used in laboratory accelerators, which require special conditions and which can as well decelerate as accelerate! There are unquestionably regions in the galaxy where the degree of turbulence, which determines the constant α, is far greater than the galactic average, and we must surely speculate as to whether such regions may not be the sources from which cosmic-ray primaries enter the galactic reservoir.

3.4. *Supernovae as cosmic-ray sources*

Of such regions, those which have attracted most attention are supernovae or, more accurately, the residual ‘ envelopes ’ of turbulent and expanding gas which persist for times of the order 1000 years after a supernova eruption. Perhaps the best-known example is the Crab nebula, which is the residue of the supernova of A.D. 1054 recorded by Chinese astronomers.

These objects emit as radio-stars, and give a non-thermal emission similar to, but much more intense than, that of the galaxy as a whole or of the Andromeda galaxy. For the Crab in particular emission intensity is relatively slowly-varying with frequency, and appreciable optical light is attributed to the magnetic acceleration of electrons with energy up to and beyond 10^{12} ev. This light is polarized, allowing the direction of the magnetic field in different parts of the turbulent envelope to be deduced. The whole situation, the scale of motion, the undoubted presence of electrons radiating in detectable magnetic fields, represents an environment where Fermi acceleration is much more likely than in the open spaces of the galaxy. Two further points support this identification: supernova residues are probably abnormally rich in heavier nuclei, while the short time they exist as recognizable entities (say 1000 years) ensures that in them the time, T, of the acceleration phase will be so short that it will not be influenced by the collision chances of the different kinds of primary. Reasonable estimates of field strengths and of cloud size and velocity, together with the observable presence of 10^{12} ev electrons actively radiating in the Crab nebula in easily detectable numbers, support the view that a primary cosmic radiation such as we receive at the earth could have originated in the supernovae which erupt from time to time within the galaxy.

Can the rate of release of accelerated particles at the ten or so supernova residues in existence in the galaxy at any one time be enough to maintain the general level of cosmic rays in the galaxy? We have a well-founded value of the energy density of cosmic radiation near the earth ((v), p. 212) and from this we may deduce a total energy within the galaxy of 10^{48} to 10^{49} joule. The average lifetime of a proton, assuming (and probably correctly) that this is limited by collision, has been estimated to be 4×10^8 years, say 10^{16} sec, while that of heavier nuclei will be correspondingly shorter. Hence, providing that the cosmic ray population of the galaxy can be regarded as in a steady state, the continuous input of power as newly injected particles must be of the order 10^{32}–10^{33} w. Now Oort and Walraven have estimated that the total energy communicated to electrons in the Crab nebula from its formation in 1054 to the present is about 10^{36} joule, which corresponds to a mean rate of 3×01^{25} w for this single supernova and so to perhaps 3×10^{26} w or even 10^{27} w for all the supernova residues existing at any one time. So unless approaching 10^5 or 10^6 times as much energy is communicated to the nuclear particles which are to comprise the cosmic-ray primaries, it would appear that these objects can hardly be responsible for the maintenance of the existing level of primary cosmic radiation. Of course the very large numbers that appear in this paragraph conceal large uncertainties. V. L. Ginzberg has proposed that the most important equation in this field of physics is $1 = 10$! It is reasonable to say that supernova residues probably contribute to and may possibly supply the whole of the cosmic-ray injection. At the moment there is no clear identification of other sources of comparable power.

3.5. *Efficiency of the galactic reservoir*

We have given little attention to one feature of the observed properties of primary cosmic rays. This is the fact ((iii), p. 212) that at the highest primary energies which we have yet had time to study or attempt to study the spectrum seems to extend upwards with no significant change of index. With some caution, the maximum point on the spectrum to be determined was described above as in the range 10^{17} ev–10^{19} ev. There are considerable uncertainties in passing from observations on air showers to the energy of the primary initiating them, and now we have to note that if the higher primary energy, 10^{19} ev, should indeed be correct, quite serious problems arise. Both in the region of acceleration, which we suppose to be small, and in the diffusing space of the galaxy magnetic fields must be large enough for the gas clouds to act, even for these most energetic primaries, as efficient reflectors. If this were not so, in the galaxy, for example, these particles would be little diffused and would escape from the galaxy much more rapidly than would less energetic particles. This behaviour would result in a deficiency of such particles in the measured spectrum, precisely what has *not* been found.

Now the diameter of the galaxy is about 10^{23} cm, and a 10^{19} ev proton would move in a circle of this diameter in a uniform magnetic field of 6×10^{-7} oersted. If the non-uniform and non-parallel fields actually existing through the galaxy are to bring about a diffusion of such particles, the maximum field strengths must be a good deal larger than this. Yet from many points of view there are difficulties in postulating fields of more than ten or twenty times this value. Current measurements of the primary spectrum are almost certainly near a most interesting limit, and the prospect of much longer experiments with extremely large detecting arrays seems inescapable. At the moment the figures just quoted leave a strong suspicion that there has been a tendency to overestimate these very high energies. The problem of fluctuations in shower growth, which may show how this has come about, and permit a more realistic interpretation, has accordingly as we mentioned above come right to the front.

Firm answers to many of the problems of the last few pages are the more elusive because we are dealing with a steady or almost steady state; the situation would be transformed in the improbable but not impossible event of a supernova eruption near (but for comfort not too near) the solar system. One may console oneself that the very steadiness of background conditions has made work on interesting local phenomena easier by far.

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The scientific revolution of the seventeenth century and its consequences†

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SUMMARY

The purpose of this paper is to consider some examples of what happens in major turning-points in scientific thinking. It is also aimed to show how the critical study of the history of science can throw light both on how science works as an intellectual discipline and on how scientists think and what they do. In critical history the analysis of history involves at the same time an analysis of science. The critical history of the scientific revolution of the seventeenth century raises the larger question of why and how the West became a scientific civilization at all. This is suggested as an ideal topic for the treatment of science as a general subject in the fifth form. It leads to several different kinds of question concerning the internal content and structure of science, the influence of general ideas, and the influence of social and economic conditions, which can be discussed from different backgrounds in the arts and sciences. But the central problem of the scientific revolution is what happened in science. There seems to have been a fundamental mutation in ideas and in the questions asked in science. These led to new techniques for answering the questions, especially by means of experiment and mathematics. These changes are illustrated by means of Galileo's contribution to the invention of the concept of inertia, Descartes' model of the animal automaton, and Darwin's theory of evolution by natural selection. In every case the turning-point appears as a result of active speculation and the manipulation and dissection of nature and critical re-assessment of basic theoretical assumptions. Of special importance is the explicit use of designed analysis by the manipulation of mathematical concepts, designed experiments, and hypothetical models.

The general question I want to consider in this paper is what happens in major turning-points in scientific thinking. The examples I shall discuss are intended as illustrations of this general theme, as well as of what happened specifically in the seventeenth century and afterwards. At the same time I want to show that to understand the history of science more than superficially it is necessary to do more than make a merely narrative study of the subject. We must study history critically.

By critical history I mean that our analysis of history must at the same time involve an analysis of science. The first object of all historical investigations must, I suppose, be to reconstruct the past. But it is one of the characteristics of science that the activities of particular men and periods yield results that are impersonal and timeless. So our analysis of the history of science must inevitably and essentially involve judgments about the relative importance and influence of different ideas and discoveries in the development of scientific thought through later generations. This certainly does not mean that we are therefore bound to read the history of science backwards, judging all past science by the standards of the present. That would be totally unhistorical and profoundly misleading. But it does mean that we bring to our analysis of history conceptions of the character of scientific thinking, techniques, and explanation which we apply as a means of comparison across different periods. I have emphasized this point at the beginning because it is in this way that the critical study of the history of science deepens our understanding of science itself. It provides in the dimension of time a larger variety of examples of scientific method and intellectual manoeuvre than very recent science alone can provide. And it provides numerous examples of the autobiography and processes of discovery which are essential for any properly inductive analysis of scientific thinking.

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When asked about the scientific revolution of early modern times, the question of what happens in major turning-points in thought raises the much larger question of how and why the West became a scientific civilization at all. Nothing similar happened in other civilizations until they were affected, mostly within the past century, by Western influence. I would like to suggest this question as an ideal topic for the treatment of science as a general subject in the Sixth Form. It certainly bridges the sciences and the humanities. And it does so not at one point only but at many, for it leads to several different kinds of questions.

It leads in the first place to questions about the internal content and structure of science. It is claimed, for example, that the main event in the Scientific Revolution was the reduction of the world to mathematics and quantity so that mathematical physics became the model for every science. This view locates the essential turning-point in the work of Galileo, Kepler and Descartes, followed by that of Newton, in developing the system of classical mechanics. A biologist or a chemist, finding the enigmas of matter difficult to reduce to this neat system, might look for other key ideas. Biologists and chemists are, as we know, traditionally somewhat suspicious of mathematics and more than a little sensitive to the overweaning pretensions of physicists. And this is not only because boys who study biology at school are mostly those who are told to do so because they are no good at mathematics. Biology and chemistry are both much less theoretical and more empirical sciences than physics. Whether, as a result, their approach to problems must always be different from that of physics is a question open to dispute.

But there are also other, quite different, questions to be asked about the Scientific Revolution. For example, about the influence of the context of general ideas. It has been claimed, for example, that the medieval attempt to make theology a rational discipline, on the model of Euclid's geometry, prepared the way for a rational science of nature, and that the separation of the Creator from the creation left the world open for treament as a rational mechanism. It has also been claimed that seventeenth-century physics is at least in part the product of the revival of Platonic philosophy, with its conception of the physical world as basically mathematical. If antecedent moves of these kinds are seen as essential conditions for Galileo's revolution in physics, then the location of the essential turning-point becomes a more complicated problem. We find our attention drawn to philosophical decisions that did not immediately yield any science, but may be considered to have been necessary before modern science could come into being. Inevitably, in any case, we are driven to admit the relevance of Greek models into the story, and to ask ourselves whether or not Greek science was in essentials the same in kind as our own and how important are the differences.

Yet another kind of question about the Scientific Revolution concerns the influence of social and economic conditions, as well as ideas, in providing institutional and personal motives and opportunities for science, and habits that encourage scientific activity. It has been suggested, for example, that one condition for Galileo's physics was the habit of weighing, measuring and accounting that grew up with the rise of early capitalist economy, and that this habit was transferred from dealings with commercial goods to dealings with nature. Support for this thesis is adduced from the fact that systems of weights and measures and arithmetic were in fact first developed extensively in business houses rather than in the atmosphere of academic learning of the universities. A more commonsense thesis, which no-one would deny, is that money and effective institutions are essential for any activity on a reasonably large scale. The foundation of scientific societies such as the Royal Society and the Académie des Sciences in the seventeenth century, the rise of their scientific members in social esteem and position, later the growth of scientific research in universities in the nineteenth century, and the recent financing of science on a large scale from public funds, are obviously all essential to the story of how the West became a scientific civilization.

Yet although we can ask these different questions about the social and ideological conditions in which the Scientific Revolution took place, they all leave us still on the outside of the central question. Favourable social and ideological conditions are not the same as the intimate association of ideas and techniques that makes up scientific thinking. The large question of how the West became scientific involves almost every aspect of civilization. But the question would not exist at all without the science. So I return to the central theme: what happened to make the seventeenth century a turning-point in scientific thinking and what were its scientific consequences?

There can be no doubt that the outstanding scientific event of the period was the new terrestrial and celestial dynamics. By the time Newton's *Principia* was published, men felt that here at last was a field in which they could confidently claim to be capable of thinking scientifically. Whatever chemists and biologists may say, I am going to accept this model and see how it was brought into operation and what use was made of it. In fact, I shall try to show that it became also the model for biology.

Why was something so obvious as classical mechanics so difficult to invent? Of course this is a silly question, because classical mechanics became obvious only after it was invented. To construct the system of classical mechanics, which we all take for granted in elementary science, meant rethinking the entire problem of motion from top to bottom. The story is one of the best known parts of the history of science, and I will simply draw attention to some of its aspects that are of particular relevance to our theme.

The way was first pointed clearly by the sublime or at least sublimely expressed—intellectual struggles of Galileo. The key physical idea for which he struggled was what we now call inertia. This became Newton's first law of motion, which states of course that a body will remain in a state of rest or of uniform velocity in a straight line unless acted on by an external force. The significance of inertia is that it became the means of analysing the whole phenomenon of moving bodies. It now seems obvious for the very reason that it was so successful.

But it was far from obvious to Galileo and his contemporaries before he arrived at this new idea. It arose from a puzzle that was both technical and conceptual. The whole phenomenon of change and motion had been given a closely-interlocking set of explanations in the current Aristotelian physics. The basic Aristotelian account of motion was that just as every continuous effect requires a continuous cause, so every moving body requires a continuous mover to keep it in motion. This account was based in the first place on direct observation. But one trouble was that it was found not to cover all the observations. For example, what is the continuous mover that keeps in motion an arrow after it has left the bow or a bullet after it has left the gun?

Besides its failure to account for such awkward facts, Aristotelian physics had for some time caused considerable intellectual uneasiness in some minds because of the assumptions it made in its approach to physical problems. Aristotelian physics aimed at offering explanations in terms of the ' essences ' or ' natures ' of natural bodies. In effect these amounted to descriptions of what bodies actually did. For example, it was the nature of a stone to fall towards its ' natural ' place at the earth and that was the explanation of this phenomenon. It is hard to describe Aristotelian physics briefly without making it seem silly. It was not silly. It should not in fact be judged by what modern physics tries to do; its aims were quite different. But for someone like Galileo in the seventeenth century, who was trying to define new aims, it was distinctly a red herring, as he said himself, in different words, on more than one occasion. So a large part of his scientific controversy was conducted at cross purposes.

For Galileo and his contemporaries the main conceptual difficulty about Aristotelian physics was that its aims were explicitly qualitative. Its qualitative system of explanations had practically no connection with measurement, calculation, and quantitative prediction. It aimed at what the Greeks called *episteme*, *scientia*, true and certain rational knowledge. This was quite distinct from what they called *techne*, *ars*, the manipulative skills, including mathematics. Thus Ptolemy's mathematical astronomy, which could predict the heavenly motions, was a distinct field of inquiry from the Aristotelian physics, which was supposed to explain them. In the sixteenth and early seventeenth centuries it was in fact widely held, by Francis Bacon among others, that the Copernican and the Ptolemaic systems were simply alternative calculating devices. They left the ' physics ' of the problem, in Aristotle's sense, untouched.

Galileo would have none of this. He tried to drop all the questions to which Aristotelian physics was the answer and to ask a new kind of question. The essence of Galileo's revolution in physics, it seems to me, was that he tried to make *techne*, ' art '—in fact the ' new experimental-mathematical philosophy ' of the seventeenth century—yield a true science of nature. The essence of physics now became measurement, the discovery of relations expressible by algebraic functions, prediction, and the quantitative testing of proposed laws and hypotheses. Aristotelian physics was not strictly testable. It now became irrelevant. And the new mathematical physics envisaged a universal system of mechanics embracing both terrestrial motion and the new astronomy.

Galileo's mutation in the conception of physics as a science seems to me the paradigm for all great turning-points in scientific thinking. He shows the mark of genius by asking new questions. We can pin-point the change precisely in his well-documented intellectual biography, for example in his work on falling bodies. His initial move was to reject any consideration of the physical cause. He said that Aristotelian ' gravity ' was merely a descriptive name. He turned instead to a purely kinematic approach. His purpose was simply to find an algebraic law describing the acceleration of bodies in free fall.

Having taken this decision, his procedure became clear cut. He began by idealizing the problem so that it became manageable. He intuitively reduced the phenomenon of free fall to its measurable essentials, distance travelled and time passed, eliminating air resistance and other accidental details and all consideration of the substance and qualities of bodies. Next he tried to invent what he called a ' hypothetical assumption ', which was a mathematical hypothesis from which he could deduce consequences that could be tested experimentally. Finally he selected the true hypothesis by specially designed experiments. So, with his famous experiments with a ball rolling down an inclined plane, he established his algebraic law of free fall, the first of the building blocks for classical mechanics.

Having thus effectively solved one important problem by means of his method of idealization and abstraction from immediate observation, Galileo now used it to tackle the question of continuing motion. For example, the motion of a projectile after it has left the projecting agent. He approached the problem through a thought-experiment. He imagined a frictionless ball rolling down one frictionless inclined plane and up another. On the second plane it would reach its original height. If this plane were then lowered the ball would have to travel a greater horizontal distance to reach the same height. If it were made horizontal the ball would travel for ever at uniform velocity as a result of the impetus of its original fall. It would in fact be moving inertially.

By means of this brilliant thought-experiment Galileo thus reached the notion of inertial motion. It is true that his conception of this fell short of that finally enunciated by Newton, but these details need not trouble us here. He used the idea of inertia in making his discovery of the parabolic trajectory of projectiles. Later, from the idealized conception of a body moving inertially in empty Euclidean space, physicists down to Newton were able to construct a mathematical theory that accounted for all the observed motions of bodies. They did so by asking themselves what happened when bodies in inertial motion collided or otherwise interacted with each other. Thus they could build up the whole mechanics of the universe from the analysis of how bodies moved in idealized simple situations. The result was the system of classical mechanics that so impressed the seventeenth-century public. In it planets, projectiles, and the ' sputniks ' which Newton himself envisaged, were all given a uniform treatment and a uniform explanation.

I have described this elementary example as an illustration of the way in which the critical history of science can illuminate the processes of scientific thought. What strikes us about Galileo's thinking is his method of idealizing problems and the ' hypothetico-deductive ' form of his procedure. Both, I think, can be seen as the result of his attempt to use ' art ', the manipulative skills of mathematics and experiment, to discover a true science of nature. In the seventeenth century this approach had further consequences that made up the Revolution.

In the first place scientific inquiry now became an active manipulation and dissection of nature instead of mere passive observation. Aristotelian science was the product of direct observation rather than experiment. Its most remarkable limitation was that it was immediately, naively empirical. In biology this yielded some excellent results. In physics it meant that Aristotle's treatment of motion never got beyond a mere classification of the various kinds of motion that he directly observed. Classical mechanics dived below the surface and found a principle with no instances that can be actually observed on land or sea. There are no actual bodies moving with inertial motion in empty Euclidean space. Actual space is occupied by interacting bodies. Yet from this non-instantial principle all observable instances of moving bodies could be reconstructed and explained. Moreover, in the light of this intellectual dissection of the problem experimental situations could be devised to compel nature to supply the answer to questions not answerable by direct observation. The scientific experiment became an active technique for anatomizing and reconstructing phenomena in the search for the laws of nature. As the philosopher Kant was to put it, the experimental scientist was not a meek pupil who accepted without question what nature chose to tell him. He was a presiding judge who compelled nature to answer the questions which he himself devised and put to her. This characteristic of modern experimental science in fact reflects a fundamental characteristic of the whole tone of Western civilization. It is characteristically activist in its attitude to its surroundings. This appears in art as much as in science. It approaches nature coercively, like a military commander, not like a contemplative philosopher. Hence the restless change, the striving for progress, so much in contrast with every other civilization.

A second consequence of this whole approach to nature was that it became an essential part of scientific inquiry to keep basic theoretical assumptions under continuous critical examination. The ideal of a scientific demonstration inherited from the Greeks was based on the notion that first principles could be seen to be self-evident, on the model of Euclid's geometry. By the seventeenth century, this had become a serious limitation. It was partly responsible for the conservative opposition to the new cosmology. In making their break with the past, the innovating party insisted on the transfer of attention from the justification of consequences by deduction from axiomatic first principles, to justification of principles themselves by their observable consequences. Hence again the significance of the manipulated experiment. And hence the notion that theories are inventions, the products not of direct intuition into the essence of things but of active ' art ' seeking to discover nature's hidden causes by inventing a language in which nature can speak and give an answer.

A third consequence of this seventeenth century conception of science as an art of discovery—a consequence of profound importance for scientific method—was the systematic introduction of the device of the hypothetical model into methods of investigation. There is no need to emphasize how fruitful this method of manoeuvre has been for every modern science, from physics to physiology and genetics. Some models had of course been used in science since ancient times. But their use in the seventeenth century introduced a new principle. It represented a complete reversal of the normal attitude to nature inherited from the Greeks.

In Aristotelian thought an absolute distinction was made between natural and artificial bodies, *naturalia* and *artificialia*. Natural things—living organisms and natural substances—were *generated* with natural principles of behaviour. It was beyond man's power to *make* a natural thing. Artificial imitations could in principle throw no light on the nature of things generated naturally. Natural generation was a mystery, concealed behind an irreducible veil. This is the view that has in fact been inherited by all vitalist conceptions of living things.

The first great modern master of the hypothetical model was Descartes. Descartes carried to the rational limit one of Leonardo da Vinci's most pregnant dicta, that to understand is to construct. His innovation was to assert the identity of the synthesized artificial construction and the natural product, and to make this identification an instrument of scientific research. Thus he could behave like an engineer in the game of searching for the nature of things.

Descartes could make this approach because of the view he held of the basic constituents of matter. He held that everything that happened in the physical world could be explained by the rearrangements of particles of matter moving according to the known laws of mechanics. Contemporary knowledge of physics and chemistry gave very little real justification for this view. It was the product of pure speculation. But it is a good example of the value of an idea held in anticipation of factual knowledge. Such ideas suggest questions of which we would not otherwise have thought. They act as guides to inquiry, and if they are any good they do yield factual knowledge discovered in their light.

Descartes' guiding idea was that the only principles operating in nature are mechanical, the same as in engineering. So he did not expect to find in natural bodies anything that could not be perfectly imitated by an artificial machine. A perfect imitation synthesized from known parts would be identical with the natural product, because the whole behaviour of both was entirely the product of the arrangement of their parts. Thus he banished all mystery from nature and threw the whole world, living and dead, open to the same form of scientific inquiry and scientific explanation. In an attractive piece of speculative writing he invites us to imagine a hypothetical world constructed according to specified mechanical principles out in distant space. He proposed it as an imitation of the actual world, and he proposed to use it to explain the actual world by comparing the two.

The most successful of Descartes' hypothetical models was his animal automaton. The value of this was that it suggested both theoretical and experimental ways of analysing the physiology of the actual body by comparison with the model. The model acted as a kind of scaffolding that could be taken away as soon as the processes of the actual body had been discovered and put into scientific form by means of its support. It seems to me that modern biology owes far more to this theoretical analysis by Descartes than to the more directly experimental approach made by Harvey. Even though Harvey was immediately right about many more facts, he provided nothing so fruitful in future investigations as Descartes' mechanistic idea.

One example of a physiological problem tackled by Descartes will illustrate the power of a model based on a theoretical idea far ahead of existing knowledge. It illustrates the obvious fact that stands out in the whole history of science, that the most important thing of all is to ask the right questions. And it shows that questions come from antecedent theoretical ideas. All this appears in the history of the problem of vision.

The Greeks dealt with part of this problem. Starting with the view that the eye is the window through which the soul looks out on the world, Euclid made an admirable analysis of the geometry of vision by taking the eye as the geometrical point of origin of the visual rays. But because of this way of conceiving the matter, no Greek writer on optics ever asked the question that is central to the whole modern analysis of vision: how does the eye operate as an optical instrument? This question became relevant only when the whole theoretical conception of vision had changed. The first requirement was the separation of the physical phenomenon of light from the psychological act of sight. Light then became something physical that entered the eye and caused sight. This implied a profound change in the whole conception both of the physical world and of the relation of the sentient being to it. The eye became a physical instrument into which information was brought. The immediate problem was the formation of the retinal image as a picture of the world. This was first raised in the middle ages and eventually solved by Kepler. But the new conception of things also raised the more fundamental problem of how the image could become information for the being in whose eye it was formed, and what happened as a result of it.

It was Descartes who took up this problem and he ruthlessly traced the succession of events that followed the formation of the retinal image. He found that he never in fact got out of the realm of mechanisms. The image was transmitted to the brain. As a result stimuli were sent to the muscles. The body then made appropriate movements. But these were built-in responses that were entirely the result of its physical construction. The same held for responses to stimuli to any other sense organ.

Thus Descartes showed that if, in the light of his mechanistic theory, he kept on asking what happened next in the succession of events following the stimulus of a sense organ, he always got a mechanistic answer. One mechanism simply set off another. The body became an automaton. Put in this way this is of course no surprise to us. But it was a great surprise and a great clarification at the time. The actual machinery of Descartes' hypothetical body now seems extremely naive. He worked out his model on the assumption that the nerves and muscles formed a hollow hydraulic system in which movements were controlled by control of the pressure of material ' spirit ' in different parts. But he used this model in the same way as modern physiology uses electronic models. The great clarification it effected was to make physiology for the first time an autonomous science, distinct from psychology, and consideration of the organism as a whole, and able to analyse the processes of the body in purely physico-chemical terms.

In the analysis of the animal body, according to Descartes, that was the end of the story. But in human beings there is of course another side to it. The retinal image gives us conscious information. We have conscious intentions and act on conscious decisions. Descartes concluded that the automaton was an inappropriate account of these human characteristics. They belonged to the soul, which he connected to the body through the machinery of the pineal gland. Here again the machinery seems naive, but Descartes' analysis of the problem was not naive. He was able to use his human model to ask questions that had never been clearly seen before. For example, he used it to make a clear distinction between voluntary and automatic behaviour in human beings. His theoretical analysis led directly to the formulation of such important physiological concepts as that of the reflex, and eventually to experimental work which, it must be admitted, had now to catch up with theory.

I have given prominence to the role of active speculation and theorizing in bringing about revolutions in science. Let me conclude briefly with one more example, the history of Charles Darwin's ideas about evolution. When Darwin's elder brother Erasmus read *The Origin of Species* he wrote off immediately to congratulate him on "the *a priori* reasoning". "It is so entirely satisfactory to me," he went on, "that if the facts won't fit in, why so much the worse for the facts is my feeling." (1)Charles' response to this compliment is not recorded, but he must have been surprised, for more than one reason. He had tried in his book to overwhelm the reader with facts. And he had also tried, there and elsewhere, to present himself as the type of scientist who is slow to speculate until forced to do so by patiently accumulated facts. Now the history of Darwin's ideas is very well documented. Why he wanted to present himself as this kind of very cautiously empirical 'Baconian' I cannot now discuss(2). But the truth is quite the opposite. He wrote himself in one of his franker letters: "How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!" (3)And in another: "Let theory guide your observations, but till your reputation is well established, be sparing in publishing theory. It makes persons doubt your observations."(4)

In fact Darwin was a highly speculative type of scientist. For example, he worked out his whole theory of coral reefs, which cleared up the whole question, while the *Beagle* was still on the west coast of South America and before he had ever seen a true coral reef. Only when the *Beagle* crossed the Pacific was he able to check his theory by examining actual reefs. He began work on evolution in the same highly speculative spirit. He knew the kind of theory he wanted when he was 28 and wrote out a sketch of it at 33. But he took his own advice about publication and waited till he was 50 before going into print.

Both the form and the content of Darwin's conception of a scientific theory of evolution are of the greatest interest. His note-books show that at the age of 28, before he had a theory, he was clear that he should look for a theory on the model of physics. He looked for a theory in which the whole production of all past and present organic forms could be shown to follow from given laws on the explicit model of Newton's mechanics. With this in mind, reading Malthus a year later suggested the idea of natural selection. He spent the rest of his life checking it against the facts.

In form, Darwin's theory of evolution by natural selection has the hypothetico-deductive structure of Newtonian physics and Cartesian physiology. In content it showed how increasing order would be automatically generated from unordered variations by the operation of purely mechanistic principles. It showed in effect how the built-in responses of a Cartesian automaton would lead it to multiply and evolve. It was an ambitious extension of Cartesian theory to the whole biological order.

"Looking back," Darwin wrote on reading the last proof sheets of the *Origin*, "I think it was more difficult to see what the problems were than to solve them."[5] Having myself never taught in a school, I confess that my experience in preparing a paper for this conference is the same. I hope that I have been able to show some of the ways in which the critical study of the history of science can help in developing an understanding both of how science works as an intellectual discipline and also of what scientists do and how they think. Of course they they do not all think in exactly the same way, but that is another matter. The peculiar value of the history of science as an academic discipline seems to me to be that it really does bridge the sciences and the humanities. It deals with thought that is impersonal and timeless. But it also deals with the men who produced it—with their gifts of imagination and reasoning, and their relation to the beliefs, aspirations, and institutions of their period. As an academic discipline the history of science rests firmly on the critical study of its own documentary sources. But it is a comparative newcomer in our academic institutions. Provided it does not fall between the different stools on which it balances in present school curricula, it seems ideally placed to provide a subject of discussion across them all.

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(3) *More Letters of Charles Darwin*, ed. Francis Darwin and A. C. Seward, London, 1903, i. 195.
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The strength of glass

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SUMMARY

Large differences exist between the theoretical and experimental values of the mechanical strength of solids. Features of this phenomenon for glass are outlined together with a review of the current, incomplete explanation.

1. INTRODUCTION

Over the past decade there has been a resurgence of interest in the problem of the ultimate strength of materials. The stress at which a particular solid should break may be estimated in various ways: most of them are rather crude but in principle are capable of giving an order of magnitude for the ultimate strength of an ideal form of the material. For example, it is argued that a simple solid material should withstand homogeneous tensile strains approaching 20 per cent before the atoms become sufficiently far apart for complete separation to occur. This leads to theoretical strengths of $0{\cdot}2E$, where E is Young's modulus of elasticity. Typically, values of a few million pounds per square inch would be expected. In practice failure of one kind or another occurs at stresses from 10–1000 times lower than this for normal samples of materials and this failure arises as a general rule through the non-ideal nature of the materials.

In crystalline solids the movements of linear irregularities in the structure that are called dislocations produce ' slip ', an inhomogeneous deformation, at stresses far below that to be expected for ideal material of perfectly regular structure. This deformation constitutes a failure of the material in a literal as well as an engineering sense since it is permanent; the body does not return to its original shape when the stresses are removed. Glasses show elastic, recoverable, deformation right up to the stresses at which fracture occurs; they are the archetype of a brittle solid. Glass is not crystalline. The atoms are irregularly arranged in space rather like those of a liquid at a particular instant in time. In the case of silica (SiO_2), which is the basic glass-forming substance for the majority of commercial glasses, several different regular arrangements and an irregular one are possible. X-ray diffraction studies have shown that in all these forms each silicon ion is surrounded by four oxygen ions arranged at the corners of a tetrahedron and the three dimensional structures are built up by the sharing of corners of neighbouring tetrahedra. Different regular patterns of the tetrahedra correspond to the crystalline modifications of silica, while the glassy form results from the irregular random arrangement of these

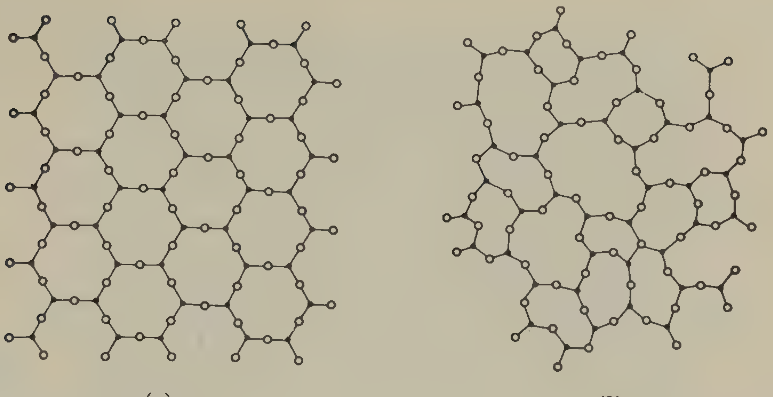
(a) (b)

Fig. 1. Schematic illustration of the structural difference between the crystalline (*a*) and glassy form (*b*) of an imaginary two dimensional oxide (M_2O_3).
The oxygen ions are arranged at the corners of triangles with the metal ion at the centroid, and neighbouring triangles share corners to form the continuous two dimensional structures.

SiO_4 groups (fig. 1). This irregular arrangement of the constituent atoms or groups of atoms makes impossible the existence of dislocations as we know them in crystalline solids, and probably accounts for the lack of plasticity in glasses at low and normal temperatures. True flow, analogous to that which occurs in a liquid, will take place in a glass at high temperatures, at which the SiO_4 tetrahedra (in the case of fused silica) move around with reasonable ease.

At low temperatures, although the glasses do not undergo plastic deformation the experimental values of the breaking stress are still very much lower than $0{\cdot}2E$. However, as we shall see, values approaching this have been recorded under special conditions.

2. Experimental strength of glass

The simplest experiments to determine the breaking stress of glass specimens can reveal the main features of the behaviour. The breaking stress may be about 10^4 pounds per square inch, but shows considerable variation from one specimen to the next; even though tests are carried out with a series of apparently identical samples standard deviations of 25 per cent of the mean are commonplace. Significantly lower strengths are recorded for larger specimens. Marked reductions in the breaking stress can be induced if the free surface of a test sample is damaged by contact with another solid. Under prolonged loading, samples fracture at a fraction of the stress required to produce immediate failure; thus the measured strength depends on the rate of loading. Efforts to obtain test specimens free from the effects of mechanical damage to the surfaces led to the use of freshly drawn fibres. Specimens in this form were found to be stronger, but high scatter in the experimental results was still apparent, and for very fine fibres the breaking strength increased rapidly with decreasing diameter. Furthermore the strength of fibres of some kinds of glass depended on the time interval between preparation and testing. Contradictory evidence on this ageing of fibres has been reported, but, for fused silica at least, the effect seems to be a real one.

Failure under prolonged load is a static fatigue quite different from fatigue in metals where failure at low stress levels occurs under prolonged oscillating stress. It has been demonstrated that fatigue in glass occurs only in the presence of carbon dioxide or water vapour in the surrounding atmosphere.

A. A. Griffith, as long ago as 1920, realized that some of these features could be accounted for in principle by assuming that there existed in the material randomly distributed localized flaws which raised the local stress above that of the average applied value. He suggested that these might take the form of fine cracks and showed both mathematically and experimentally that the stress at the tip of a crack exceeds the average stress by a factor which depends on the dimensions of the crack. This stress-raising factor can be quite large even for fine cracks which would not be readily visible. The ease with which plastic sheet, as used for food containers and for some rainwear, will tear once a ' crack ' has been introduced provides a familiar everyday example of the role played by a crack in raising the stress.

The existence of flaws of this kind could account for the failure in tension at low values of stress, and for the high scatter as well as for the variation in strength with specimen size. In a simple tension test, fracture would occur at the most severe flaw in the particular specimen. The variation in breaking stress with sample size is then attributed to the higher statistical chance of having a severe flaw, for example a relatively large crack favourably orientated, in a larger specimen. The scatter of results from a group of similar specimens would also reflect the statistical variation in the severity of the worst flaw in the individual specimens.

Originally Griffith suggested that flaws of this type might explain the low strength of metals as well as glass. For metals other than cast iron this hypothesis has been abandoned. In cast iron the excess carbon in the material is precipitated out under normal conditions as small elliptical flakes of graphite and it is these inclusions with different elastic properties which act as stress raisers. In the case of glass no direct evidence has been recorded for the existence of real cracks in an undamaged surface and until recently it has been common to suppose that the stress raising flaws were very fine invisible cracks.

The fracture surface of a glass specimen is usually marked in a characteristic pattern. Although the reasons for this are not fully understood it does provide very useful additional information about the process of fracture of a specimen. The area of surface close to the point of fracture is usually quite smooth to the naked eye and even under normal optical microscopy. This so-called mirror surface is surrounded by regions of progressively increasing roughness out to areas where forking of the fracture surface has occurred leaving quite pronounced ' mountain and valley ' structures radiating as if from the point at which fracture started (fig. 2). This is of considerable importance technologically, for an examination of the surfaces of a fractured sample can often reveal the site at which failure started. It has contributed useful information to the fundamental problem since examination of the fracture surfaces of test specimens reveals that in most cases fracture starts at the surface of the specimen in a region that is under tensile strain. That failure occurs in tension from flaws at the surface is turned to good account in making ' toughened ' glass which is now in common use as car windscreens and ' unbreakable ' tumblers. Glass articles are toughened by chilling the free surfaces of the finished article, usually with blasts of

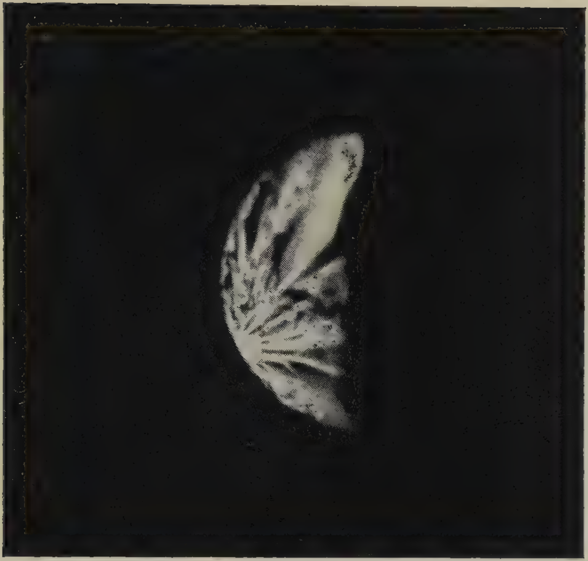

Fig. 2. Fracture surface of a fibre of Pyrex glass. (The smooth mirror region which occurs around the point of origin of fracture is often larger than in this example.) (×150.)

air, from temperatures close to the softening point of the glass. The outside layers are thus ' set ' while the central layers are still hot enough to relax by true flow; as this internal core cools more slowly and contracts, the outer chilled layers are put into compression. There exists then a marked stress gradient through a cross section of the article. In normal use much higher applied stresses can be achieved before the surface is under a nett tensile stress large enough to produce fracture, although once fracture is initiated, or if the normally compressed surface layers are disrupted the material will shatter completely into small particles or fine powder depending on the magnitude of the stresses induced by the chilling of the surface.

3. Recent work on the strength of glass

The commercial interest in the mass production of glass fibres for sound and thermal insulation as well as many other purposes led to the development of methods for producing fibre in the form of a continuous filament at high speed and in large quantities. In this process fibre is drawn through small holes in a crucible containing very hot fluid glass. Recent measurements on samples of fibre prepared in this way have shown that, provided great care is taken to collect samples and test them without mechanically damaging the surface, high and very consistent strengths are obtained. Moreover, there is no variation in strength with the diameter of the fibres. The consistency of these results is particularly interesting and it is tempting to infer that samples prepared in this way are free from stress raising defects, although the magnitude of the breaking stress (5×10^5 pounds per square inch) is still rather lower than is expected from simple theoretical calculations. On the other hand there are results which, although not yet fully substantiated, are enough to cast doubt on the possibility that the ultimate strength has been attained. Samples of silica fibre can be produced by simple drawing techniques which have strengths of about 2×10^6 pounds per square inch. Furthermore there are reports of an occasional sample of the lower softening point glasses with strengths in this region. However, it does seem that a major source of stress-raising defect has been effectively eliminated from fibres drawn at very high temperatures.

Some work which we have been doing in this laboratory has revealed the nature of these defects. We have discovered that it is possible to detect with the ordinary optical microscope some flaws on the surface of fibres which have been drawn from rod at low temperatures (fig. 3). We have proved that fracture starts at or very close to these visible defects (fig. 4) and also, by testing very short lengths of fibre, that the surface between defects will sustain stresses up to 5×10^5 pounds per square inch. We have demonstrated that these visible defects are formed during the heating of the rod preparatory to drawing by interaction of the glass with dust particles or other contamination present on its surface. Flaws similar both in appearance and effect can be produced by annealing, at temperatures below the softening point, a fibre which has been contaminated with small particles.

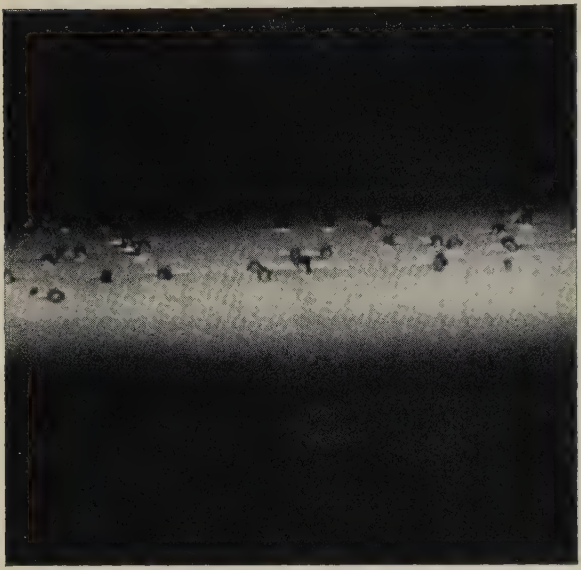

Fig. 3. Defects on the surface of Pyrex glass drawn from rod at approximately 920°c. (×175.)

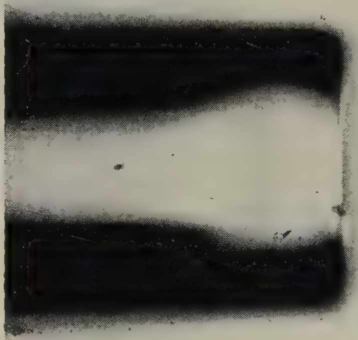

Fig. 4. Defect at the edge of the fracture surface on a fibre of Pyrex glass. (×150.)

It has not been possible to avoid the formation of these defects by cleaning the rod by any simple process before heating. However we have been able to demonstrate that even after deliberate gross contamination of the original surface very strong fibres with no visible flaws can be produced by heating a short length of the rod to a high temperature immediately before drawing. It seems likely that the high temperature treatment leads to complete dissolution and mixing of the contaminating material thus precluding the formation of extreme inhomogeneities on the surface of the fibre. This could also explain the high strengths of fibres drawn from crucibles at high temperatures. There is no reason why this type of flaw should be confined to fibres; in fact with the usual conditions prevailing in a glass works contamination of the surface of a fresh, hot glass surface by air-borne dust would be almost certain.

We are now trying to find out how these local inhomogeneities act as stress-raisers. It is conceivable that the basic mechanism is similar to that in cast iron!

At the present time, then, we can recognize that major flaws on the surface of glass can result either from mechanical damage or from the presence of foreign particles, although in neither case do we yet understand the mode of operation. When these flaws are eliminated specimens of consistent high strength are obtained. It appears that this high strength still falls short of the ultimate strength of the material and it is possible that a third type of imperfection is involved. Several groups of research workers are now examining the properties of the very strong fibres. Do these show ageing or static fatigue, are they influenced by their environment or is this behaviour characteristic of the flaws rather than of glass itself? If it is necessary to invoke a third type of defect, will this prove to be inherent to the structure of glass or does it like the others arise through external interference? Solutions of these problems are not only of academic interest. Technological progress depends intimately on the extension of the range of materials available. Even with breaking strengths of half a million pounds per square inch, the strength to weight ratio of glass will make it a very attractive material to the design engineer, once we have discovered how to prevent the production of flaws by mechanical damage.

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Television and the teaching of science

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Summary

Television can be used in the service of informal education, or more formally in schools and colleges. This article deals with the educational problems raised by the latter use. Those problems will only be solved when scientists in general take the medium seriously enough to master its techniques of communication and to co-operate in assessing its impact. Its powers and limitations, which are not basically different from those of film, can already be discerned in broad outline. The ways in which it is being developed in different countries depend not only on the qualities of the medium but on their differing educational systems. In this country the emphasis is on the help which television can give to the teacher at all levels rather than on the possibility of replacing him. The nature of that help must be determined in the light of educational purposes.

I have been asked for a broad survey of the questions raised by the uses of television in education. It is easy enough for a layman concerned with the development of the medium to raise those questions; the answers must come from scientists, whether as teachers, or as practitioners who have mastered the techniques of a new means of communication, or as research workers in the field of educational psychology. In the meanwhile, television goes ahead tentatively and in the dark, whether it is addressed to the evening audience or to smaller but more homogeneous groups in the classroom. It is with this latter aspect of the problem that I propose to deal. There can be little doubt that programmes like " Eye on Research " and " Science is News " with their audiences of 3 or 4 million people are the most powerful instrument that has existed for making those with a modicum of scientific knowledge aware of recent developments, for the widest possible diffusion of scientific ideas, for creating in the public mind an image of the scientist as something other than an inexplicable wonder-worker, for showing the social impact of science, and perhaps, for conveying to those without any scientific background something of the spirit and methods of scientific enquiry. Evening programmes are handicapped, and will be until more television channels are available, by the heterogeneous nature of the audience, and by the fact that it is not possible to start from any firm assumptions about its existing background of knowledge. In this country they have not yet been used to teach science, or to contribute to education in the more rigorous sense of the word; that is, to the cumulative mastery of any part of the field of knowledge.

In whatever context it is used the methods of the medium remain the same, and fundamentally they are not new. Television uses moving images as the scientific film does, and faces the same problems. Film tends to be perfectionist in its approach and impersonal in its address, while television is rough-and-ready, but more intimate, aiming to establish a personal relationship between the expositor and his audience. These, however, are customary or accidental rather than inherent differences. Film is, in fact, one of the most important ingredients of television; the crucial experiment and the factory visit can be more safely entrusted to film than to the hazards of the ‘ live ’ studio demonstration or the outside broadcast; and when programmes are pre-recorded in the studio complete with ‘ intimate ’ narration and filmed inserts there is no appreciable effect on the response of the audience. The older medium offers some obvious advantages to the teacher. A film is completely under his control; it can be used when he needs it, and repeated; television programmes have compensatory advantages in the case of distribution. For material and economic reasons they lend themselves more readily to planning in series, supported by ancillary literature, and they are likely to be available for many years to come over a much wider range of subjects, with a greater precision and variety of aim than film, and drawing on wider resources. Those resources can be easily listed. First of all come the scientists themselves. There have been experiments in the visual presentation of scientific material with a spoken commentary but without any expositor in vision. The method calls for a careful comparative assessment, and a parallel research might go some way towards breaking down the vague concept of ‘ personality ’ and showing what contribution the living speaker makes towards understanding, and in what ways. It seems likely, however, that a personal presentation will remain the dominant method. There will therefore be an increasing demand for scientists who know the medium, either as teachers using every device of pace and emphasis, of expression and gesture; or as specialists speaking with the authority and conviction that come from personal experience.

They will have at their disposal not only the whole range of existing resources, but such specially made models, charts, animated diagrams and films as they require, and an instrument with an unrivalled capacity for close and detailed demonstration. A model of Hero’s aeolipile or Hertz’s apparatus, an automatic telephone switchboard or a twenty-foot wind tunnel will be as readily available to them on film or on the studio bench as a Wheatstone bridge in a school laboratory. In chemistry the absence of colour imposes the sharp limitations of a world in which neither litmus nor Fehling’s solution can be effectively used; but that is a handicap which will one day be surmounted. In physics the possibilities are not similarly restricted.

Those possibilities cannot be fully exploited by the scientist who is content to think of television as a means of making available to a wider audience his customary lecture or laboratory demonstration, or of presentation in terms of the medium as something that can be left to the producer. They are waiting to be developed by those who are willing to master the intricacies of the medium, who know its limits, but who have ceased to ask whether its characteristic devices are any more than what Sir Joshua Reynolds, in another context, called “ a provision of endless apparatus, a bustle of infinite enquiry, employed to evade and shuffle off real labour—the real labour of thinking ”.

There is a sense, perhaps, in which that is still a fair question to ask. At the studio end the very richness of his means embarrasses the programme planner. He has to impose coherence and unity on widely diverse elements. He is forever tempted to include the fascinating digression, the illustration so striking that it will draw attention to itself rather than to what it illustrates. With sixteen horses at his command he wonders whether there might be advantages in showing the effect of atmospheric pressure as Von Guericke did and not with the rather simpler laboratory apparatus. With the visual aspect of the programme uppermost in his mind he is tempted to believe that visual images in careful sequence can by themselves communicate a train of thought, and to forget that he brings to their contemplation a framework of concepts that will only exist in the viewer's mind if it is clearly conveyed to him by the verbal commentary. At the viewing end this same richness and variety of means creates ' interest ', but that may be no more than a high degree of voluntary attention to a succession of unrelated images. As yet we have developed no adequate techniques for either ensuring, or assessing, comprehension of the whole. We know very little about the optimum relationship of words to images or the optimum span of attention in given contexts and for given audiences. We have hardly begun to find out in what circumstances and to what extent television can speed up the learning process, lead to longer retention of knowledge, or act as a stimulus to further study. It may well prove that where spatial relationships are concerned or where spatial analogies are valid, the new medium will serve as an avenue to comprehension for children and adults who have been handicapped by a lack of verbal ability. We do not yet know.

Some things, however, can be said with a fair degree of confidence. In the descriptive branches of science a successful television programme can more or less directly extend the experience of the viewer. Monochrome images on a small flat screen have obviously to be interpreted, but they can bring to the Nature Study class in the primary school as well as to the zoologist, Frisch's dancing bees or the tortoises of the Galapagos islands. The evidence suggests that these are a stimulus to, and not a substitute for; first-hand observation of the natural world. Again, television can by a synthesis of resources offer the viewer a synthetic view of a wide network of relationships. It can demonstrate a principle, show its exemplifications in everyday life and its large scale industrial applications, and thereby bring home alike to evening viewers and to children in secondary modern schools the social significance of scientific research.

As for the explanatory branches of science, television must proceed in the faith that has inspired the brilliant work of Sir William and Sir Lawrence Bragg at The Royal Institution, which now happily reaches a yet wider audience through television. It must assume that skilled and effective demonstration can give the competent viewer a general understanding which can usefully precede the precision of a quantitative approach and which will be a stimulus to the attaining of that precision. But television can, of course, take the viewer no further than the art of demonstration with flat images and appropriate words can go. How far that is can perhaps be seen by considering the role of the animated diagram and the model. There are diagrams that gain nothing from animation, like those of geometrical optics. To show light as ' travelling ' would add nothing to their explanatory value. They can best be studied on the printed page. But there are others that must be ' animated ' by the mind if they are to yield their meaning. Animated diagrams can therefore have a directness and clarity that makes them invaluable for teaching purposes. The more complex the process or system of concepts involved, the greater the service they render. A verbal explanation must often distort reality by its linear character, since, as von Haller put it, " Nature has linked her kinds into a net, not into a chain; men are incapable of following anything but a chain since they cannot express in words more than one thing at a time ". An animated diagram showing, for example, a feed back loop in a self-regulating system or the simultaneous processes of respiration and photosynthesis in plants, can restore the complex network.

It is over analogical diagrams and models that difficulties begin to arise. Lord Kelvin who was " never happy until he could make a mechanical model of a thing ", and for whom model-making meant stringing together little balls and beads with sticks and elastic bands to illustrate crystalline dynamics would have found in television an ideal medium. It offers unlimited scope for those whose expository gifts and ingenuity in creating visual devices go hand in hand. But though the modern physicist may be willing to use analogies in exposition he is always conscious of the dangers of conveying a false or oversimplified impression by presenting abstract relations in visual terms. The ' models ' which he uses in his own work are likely to be purely mental constructions, and the inferring techniques on which he depends make use of a sophisticated mathematics. Those who would follow him beyond a certain point must leave picture-thinking behind, and enter a more abstract realm, where television has nothing to offer but a wider audience for the speaker and his blackboard. These then are some of the powers and limitations of television. How it is to be used in the service of education will be decided by the educational world in the light of its own purposes and aims and its estimation of the relative value to be assigned at different levels to the various elements of science teaching—the practical work of the laboratory, the dialectic of the classroom, and the skilled demonstration. In any event the task of providing a service of television programmes that can be effectively used in schools or colleges is a formidable one. It can be approached in widely different ways.

In the United States, where the shortage of science teachers is even more acute than ours, there have been various attempts to take over the whole burden of teaching. The National Broadcasting Company last year launched the series " Physics for the Atomic Age ", which took the form of one hundred and sixty half-hour lectures by Professor Harvey White of a straightforward and efficient but conventional kind. They were broadcast at the rate of five a week (and at 6.30 a.m.) and addressed in the main to High School physics teachers anxious to improve their qualifications. At Compton Junior College in Los Angeles the whole of the instruction in algebra and some other subjects is given by means of filmed lectures broadcast on a closed circuit, an extension of automation to the teaching world which has already evoked a Luddite protest. In Hagerstown the Ford Foundation is pouring money into another closed circuit scheme designed to place the best local teachers at the service of all the local schools. In none of these instances does the programme amount to much more than a televised classroom lesson. The claims made for these experiments are sweeping, and it is alleged that students taught solely by television do as well as or better than those taught by conventional means. Professional opinion in this country is sceptical both about the validity of American multiple-choice tests and about the merits of American teaching by conventional means. In any event these ventures are unlikely to find imitators. So long as the various broadcasting organisations are each of them confined to a single channel none is likely to devote so large a slice of broadcasting time to a minority audience as N.B.C. has done; and an experiment in providing a complete science course for even a single year in a single type of school would absorb nearly half of the generous but still limited time and resources which the B.B.C. has allotted to school broadcasting.

In France, educational television is the responsibility of the Ministry of Education, and the output is designed to supplement and reinforce the official syllabus for each age-group at a number of carefully selected points. It is an approach which is only practicable in a highly centralised educational system of the French type with the schools working to a rigid time-table.

In Britain, the B.B.C. under the guidance of its School Broadcasting Council is pursuing three lines of approach, which will be fully developed in the school year 1960-61. There will then be a regular series of Nature Study programmes for the 10–13 year olds. There will be two series of programmes for the Secondary Modern School, where the shortage of teachers is most acute. One of them will be designed to help the teacher with only modest qualifications in science. It will provide a weekly basis for his work with first year groups as a text-book might do, but will enlist his active co-operation in following up the programme during the rest of the week. The other will supplement the work of the science teacher for the older groups both by showing science at work in its social context, and by offering series of programmes in fields like astronomy that lie outside the usual range of school studies.

Associated Rediffusion Ltd. provides a series with broadly similar aims and is experimenting in a second series with programmes designed to encourage the growing tendency to introduce some study of the physical sciences at the top of the primary school. But it is perhaps the sixth form in the Grammar School that offers the greatest challenge and the greatest opportunities. Granada Television has already made a start with that audience and the B.B.C. series planned for the Spring Term of 1960 is intended to lead to a regular provision. At that level there are three tasks; the opening up of new horizons for the science specialist; the building of bridges between the Arts and Science sides; and the scientific education of the Arts pupil. To the science specialist television can offer the latest findings of research interpreted by the pioneers in the field who can convey to him not only the rigours but the high imaginative quality of scientific thinking; for the whole sixth form together it can provide an introduction to fields of discourse such as evolutionary theory or atomic physics which find no place in the conventional syllabus, but which are socially or philosophically of wide significance; for the Arts student there might be designed an introduction to scientific thinking which would help to relieve the overburdened science staff. It could be focussed on science in its social context. It could take Conant's line and embark on a study of the strategy and tactics of science as exemplified in such classic researches as those of Robert Boyle. It could even go half way to meet the opinion of those who think that the most valuable provision for the Arts Sixth would be the continuation of some straightforward science teaching. There can obviously be no substitute for individual laboratory work where the making of scientists and scientific technicians is concerned, but many of us will never in that sense be scientists. It is perhaps worth remembering that the Thomson Committee came to the conclusion that " the view that experiments by the class must always be preferred to demonstration experiments leads to great waste of time and provides an inferior substitute ", and that in Sir William Ramsay's opinion " it is possible to have quite an intelligent idea of chemistry without ever having handled a test tube or touched a balance ".

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The Science Masters' Association Annual Meeting

by H. LOCKETT

Taunton's School, Southampton
(Annual Meeting Secretary, S.M.A.)

The Annual Meeting of the Science Masters' Association, held in the University of Southampton from Tuesday, 29 December 1959 to Friday, 1 January 1960, was attended by some 600 members, of whom over 300 were accommodated in three Halls of Residence.

Just over 400 members of the Association and their ladies were welcomed at a Civic Reception in the Guildhall on the evening of Tuesday, 29 December, by the Worshipful The Mayor of Southampton, Alderman Mrs. R. M. Stonehouse, J.P. The reception was followed by a lecture on ' Southampton Borough Archives ' by the Borough Archivist, Mr. B. C. Jones, M.A., and during the evening members were able to examine some of the historic treasures of Southampton, together with models and illustrations of modern educational buildings, prepared by the Borough Architect, Mr. L. Berger, Dip. Arch., A.R.I.B.A.

On Wednesday evening, 30 December, the Association Dinner was preceded by a University Reception, at which some 400 members were received by the Vice-Chancellor, Dr. D. G. James. There was a very large and appreciative audience for the Presidential Address by Dr. James, whose subject was ' Science and Literature '.

A full programme of lectures had been arranged for the morning sessions. These included

(1) ' Some demonstrations in Light and Colour ' by Professor A. M. Taylor, M.A., Ph.D., F.Inst.P.; a series of beautiful experiments suitable for demonstration in schools, showing the production of spectral, additive and subtractive colours; colours by incoherent scattering, coherent diffraction and interference; subjective effects of dichroism, contrast colour and after image.

(2) ' Directed valency ' by Dr. G. W. A. Fowles, Senior Lecturer in Chemistry: a development of the ideas based on hybridization and its applications to the simpler compounds of the light and heavier elements; considerations of symmetry and deviations from ideal symmetries in terms of electron repulsion for bonding and non-bonding pairs, multiple bonding and the ionic character of bonds.

(3) ' The solid state ' by Mr. S. Weintroub, M.A., F.Inst.P., Senior Lecturer in Physics: an analysis of the theoretical approaches to solid state physics, with results of some recent investigations into the effects of imperfections on the physical properties of crystals.

(4) ' The application of the Pegasus digital computer to the solution of problems in plant ecology and Taxonomy ' by Professor W. T. Williams, A.R.C.S., Ph.D., D.Sc., F.L.S.: a method of using the digital computer in the survey of a new area and to detect discontinuities by observation on individual species.

(5) ' Some new problems in Aeronautical Research ' by Professor E. J. Richards, O.B.E., M.A., D.Sc., F.R.Ae.S.: introducing new concepts in the treatment of Engineering problems with reference to the study of noise arising from fast turbulent streams and to the properties of gases at very high speeds.

(6) 'Analogue computers and their use as a teaching aid ' by Mr. B. H. Venning, B.Sc. (Eng.), A.C.G.I., A.M.I.E.E., Lecturer in Electronics: the basic principles of analogue computers and how they can be used to explain the fundamental systems encountered in the teaching of mechanics, acoustics and electrical circuits.

(7) ' Modern developments in the use of cooling in physiological and biochemical studies ' by Dr. K. A. Munday, Senior Lecturer in Physiology: a review of the effects of body temperature and of current research into hypothermia and tissue culture, with a consideration of the use of cold as a quantitative means of studying the reaction of animals to unfavourable circumstances.

(8) ' Reactions of " hot " molecules ' by Dr. H. M. Frey, Lecturer in Chemistry: a new method of producing and studying nearly monoenergetic highly excited molecules discussed with reference to theories of molecular reactions.

(9) ' The Junction Transistor and its application ' by Mr. E. Wolfendale, B.Sc., A.M.I.E.E., Chief Semiconductor Applications Engineer, Mullard Radio Valve Co.: an analysis of the theoretical basis of the junction transistor, with examples of its application to domestic, communication and control equipment.

(10) ' Nuclear Engineering—Past, Present and Future ' by Dr. N. S. Grassam, Lecturer in Mechanical Engineering: an explanation of the work of the nuclear engineer dealing with topics such as the choice of reactor and problems associated with particular types of reactor.

(11) ' Research on Game Birds ' by Mr. T. H. Blank, B.Sc., Biologist to the I.C.I. Game Research Station, Fordingbridge: an analysis of the factors which control game-bird population and the development of artificial rearing methods with reference to an accurate census over a period of ten years of partridges on a 4000 acre area.

The visits programme has proved to be as popular as in former years, and on the afternoons of Wednesday, 30 December and Thursday, 31 December nearly 500 members were able to visit works and places of scientific interest in the vicinity. The programme included visits to the Esso Petroleum Co. Ltd., Southampton Docks, the Union Castle Liner ' Winchester Castle ', International Synthetic Rubber Co. Ltd., Pirelli General Cable Works, John I. Thornycroft & Co. Ltd., Winchester College, Hampshire Cattle Breeders Society, British Drugs Houses, Saunders-Roe Ltd., Marchwood Generating Station, Mullard Ltd., and H.M.S. ' Collingwood '. During the afternoons, parties were also conducted round some of the University Departments, and were able to inspect the tidal model of Southampton Water and the Computer Department.

The exhibitions have always been an important part of the Annual Meeting, and the ingenuity shown in the Members' section of 60 exhibits should have afforded welcome refreshment to the many science masters who were present. Manufacturers put on an excellent display in the Chemistry Laboratories, and Publishers were very well represented in the new luxury of the Lanchester Building, recently completed to the design of Mr. Basil Spence, P.R.I.B.A.

Through its Secondary Modern Schools Committee, the Association has been increasingly concerned with the furtherment of education in science in secondary modern schools, and a new feature of the Annual Meeting was a special exhibition of apparatus and teaching aids specially suitable for secondary modern schools—much of it incidentally of value to teachers in grammar schools as well. The nucleus was provided by members of the Modern Schools Committee of the Association, supplemented by exhibits from other members and including a notable contribution from the Hampshire schools. In connection with the exhibition, a demonstration of some of the equipment was arranged for Wednesday, 30 December in the Institute of Education.

In following up the implementation of the ideas expressed in the Policy Statement of the Association, ' Science and Education ', various committees have considered the importance of some knowledge of the structure of the atom and nuclear energy being acquired by all pupils before they leave school. One result was seen in the special exhibition on 'Atomic and Molecular Structure and Nuclear Energy' in the Assembly Hall, designed to display equipment suitable for use in schools on this theme, and to increase the background knowledge of science teachers in the subject. A number of organizations and industrial firms in the United Kingdom contributed material, including the Royal Institution, the Cavendish Laboratory, The Atomic Energy Research Establishment and the Central Electricity Generating Board. The major industrial displays included a student reactor and monitoring equipment and a number of fine demonstrations.

Scientific knowledge knows no territorial boundaries, and the Association is playing an increasing part in developing co-operation in international affairs. In this exhibition we were very happy to include apparatus which had been sent from America under the auspices of the Apparatus Committee of the American Association of Physics Teachers, including the determination of h/e, an audio and video alpha particle counter and a microwave adaptation of the Bragg diffraction experiment, demonstrated by its contributor, Professor Harry Meiners of New York. Another pleasing feature was the display of equipment available to schools in Western Germany, demonstrated by Dr. Hecht, who put the Association further in his debt by his lecture on 'Atomic Physics in West German Schools' on Wednesday, 30 December.

A programme of films was arranged for Wednesday and Thursday afternoons in the Institute of Education, supplemented by a special exhibition on visual aids which shared quarters with a feature display on Textile research by the Department of Scientific and Industrial Research.

In addition to the discussions which inevitably go on round the Common Room fire, often well into the small hours, more formal topics were presented on Friday morning at simultaneous discussions on the draft syllabuses in Physics, Chemistry and Biology, prepared by subject panels of the Association and arising from the policy statement referred to previously. Arrangements had been made for a party of members from the I.A.A.M. Council, meeting in Southsea, to join the Science and Religion Group discussion on Thursday, 31 December. The discussion was led by Dr. MacKay of King's College, London, his subject being 'The Christian Doctrine of Man'.

The Institute of Physics Report on the teaching of mathematics to physicists (1959)

The original report of this title was published in 1944 and was written by a joint committee of the Mathematical Association and The Institute of Physics under the Chairmanship of the late Professor J. A. Crowther.

In the present report, prepared by a committee appointed by the Board of The Institute of Physics, considerable revision has been made in the light of experience and of what is believed to be the need of the immediate future.

In the past there was a feeling amongst physicists that the type of training in mathematics received by students of physics failed in some respects to meet the requirements of the times. The first version of the report as issued in 1944 was an attempt to suggest principles which it was thought should determine the method of teaching mathematics to prospective physicists in university courses. The following account summarizes the 1959 revision.

For the average student of experimental physics, the full discipline demanded of a mathematical specialist must be mitigated. The problem is not a new one, but it is becoming of greater urgency with the increasing extension of the content and technique of both mathematics and physics.

Mathematics may often be more easily learnt and more confidently used if presented as the quantitative and generalized form of logical processes familiar in the world of physics and, in fact, often considered as ' common-sense '.

The right approach to the student's difficulties may produce surprisingly effective results but a student whose mathematical ceiling is really unduly low would be well advised to transfer his main interest from physics to some other branch of science.

The report is not directly concerned with the teaching of mathematics at the elementary or school level.

It is not suggested that the kind of mathematics taught to physicists should be different from the kind of mathematics taught to mathematicians. The difference is in the amount of time available. It is recognized that with all the will in the world a physicist cannot learn as much mathematics in the three or four years of his undergraduate course as can his mathematical colleague.

It is this fundamental understanding that should be taught at the expense of devoting less time to the technique.

It is essential to teach physicists certain mathematical techniques, but it is even more important that they should learn how mathematics is used in physics. Too often students do not realize that the mathematics only expresses the physical ideas on which a theory or model is based, the first step in approaching a problem.

The next step in the process is often comparatively simple; it involves the application of mathematical techniques to get what is often called the result or the solution. This in turn has to be translated back again in terms of the physical characteristics of the problem.

The first and third of these stages require as much emphasis in the teaching of applied mathematics as they do in the teaching of physics; their neglect can be disastrous for the students.

A physicist must learn a good deal of mathematics after graduating. One purpose of his undergraduate training in mathematics should therefore be that of guiding him to a stage from which he can extend his knowledge by his own reading without undue difficulty.

The idea of rigour and the idea of generality are often confused in mathematical expositions intended for physicists. There is often a good reason for sacrificing generality; there is never a good reason for being satisfied with anything less than complete rigour. The way to avoid undue difficulty in a proof is usually to state the theorem under more restrictive conditions than the most general under which it is valid. Even with such restrictions the theorem is often sufficiently general for a physical application. Indeed, it may be uneconomical and is sometimes pedantic to insist upon the utmost generality in the statement of a theorem which is needed for a specific physical situation. This, then, is the real difference between the attitude of a physicist and that of a pure mathematician. The former may be satisfied by a proof which is valid in a special case, while the latter is never completely satisfied unless the utmost generality is attained. A non-rigorous proof, however, is of no use to either.

The broad aims of the mathematical education of a physicist should be to enable him to read, after graduating, the important books in his subject and to use the literature to help him to solve the problems that arise in his chosen field of work. He should be able to use books on the methods of mathematical physics such as those by Jeffreys and Jeffreys, and Courant, and Hilbert, and later be capable of proceeding to more advanced works on theoretical physics such as the classical books on quantum mechanics by Dirac and von Neumann.

Since the physicist will be studying mathematics mainly for the purpose of applying it to his own particular problems, the distinction which is often drawn between pure mathematics and applied mathematics becomes meaningless in his case, and may actually be misleading.

It is hoped that this unification of the subject for the physics student may be useful, not only in relieving him of possible misconception, but also in allowing him to spend the time allotted to mathematical studies in the most profitable manner. It should be kept in mind by the mathematical teacher that much of the time of the physics student will be spent in applying mathematics to problems which arise naturally in his own subject, and that he will obtain a considerable amount of practice in problem-solving in this way.

Particularly since the war there has been increased co-operation between university departments of physics and mathematics in the devising of courses for physicists and in the manner of their presentation. The mathematics department should be responsible for these courses and the lecturers giving them should be members of the mathematics staff. The report is not in favour of mathematics being taught to physics students by physicists.

Since the student will wish to acquire the maximum knowledge of useful mathematics in the limited time at his disposal (roughly one-third of that available to the student specializing in mathematics) the syllabus should be drafted with that object in view. The original version of this report included suggested syllabuses in the form of three schedules. The present one gives a single syllabus which is probably somewhat wider than could be covered in the usual course for the Honours physics degree, recognizing that teachers would need to make some selection from the " applied " mathematics topics.

This syllabus includes some numerical analysis, for the education of a physicist can hardly be considered complete without some knowledge of numerical techniques and machines. How much can be included in a course for undergraduates will depend on the time available but it is urged that physicists should at least be introduced to simple error analysis, finite differences, interpolation, numerical differentiation and integration, and applications of these techniques to the solution of algebraic and differential equations.

The report concludes with a selection of questions set in the Institute's Graduateship Examination.

Copies of the report are obtainable from the Institute of Physics, 47 Belgrave Square, S.W. 1, price 2*s*. 6*d*.

D.S.I.R. Grants

The Department of Scientific and Industrial Research is now supporting nearly twice as many postgraduate students in science and technology as it did some three years ago. The research workers receiving D.S.I.R. studentships this year number 2143 (1301 in 1957); a further 271 (152 in 1957) are holding advanced course studentships; there are also 48 D.S.I.R. Research Fellowships (18 in 1957). Some of these awards are being held abroad. The cost of this programme is expected to exceed £1 000 000 during the coming fiscal year.

The D.S.I.R. has also awarded nearly a million pounds more than in the previous year for special researches at universities, technical colleges and similar institutions. In the past year these grants, totalling £2 238 000, have been made to 287 investigators for researches in the natural sciences, human sciences and technology.

Further information on these awards is contained in two D.S.I.R. booklets published in January 1960 by H.M.S.O. They replace the former ' Notes on D.S.I.R. Grants to Graduate Students and Research Workers ' and appear under the new titles—' D.S.I.R. Studentships and Fellowships ' (price 1*s*. 6*d*.) and ' D.S.I.R. Research Grants ' (price 1*s*. 3*d*.).

Foundations

Copies of the I.A.P.S. booklet "Foundations", which was discussed in our December issue, are obtainable from R. A. Harrison, Snape Hall, nr. Saxmundham, Suffolk, price 2*s*.

The Physical Science Study Committee
(1) A survey of its publications

by J. L. LEWIS
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In 1956 a committee was set up in the United States under the chairmanship of Professor J. R. Zacharias of M.I.T. to study the teaching of physics in secondary schools and to suggest such changes as might be desirable. This committee soon decided that a thorough revision of the content of the physics courses in schools was needed and that there was also a need for a completely new approach to the subject. Their reasons for this were:

(i) that secondary school physics textbooks in general presented the subject from a point of view that dated back half a century and " no longer reflected the views of the scientific community ",

(ii) that genuine attempts to add new material had resulted in a patchwork quality in which *the unity of physics* had disappeared,

(iii) that the amount had become so great that it could not be reasonably taught in an academic year,

(iv) that much of the new material reflected the increasing importance of technology rather than the introduction of new scientific concepts.

For these reasons the Committee decided to draw up a completely new syllabus, to write a new textbook and a laboratory manual, to provide films and filmstrips, to publish a series of monographs supplementing the course, to write a special teacher's guide, to provide apparatus both for demonstration and class work, and to run courses for the guidance of teachers before they use the new material.

The general aims of the programme were (1) to provide a good scientific background for the section of the population which passes through the secondary school system and (2) to develop a physics course that emphasizes " the essential intellectual, aesthetic and historical background of physical science ". (This latter aim may perhaps be a little misleading to English ears: there is no question of the course being one on the History of Science. The emphasis is more on scientific method revealed through a limited number of basic topics than on the acquisition of much factual knowledge.) It was *not* specifically intended to prepare pupils for university physics.

The scope of the work undertaken by the Physical Science Study Committee can thus be seen to be immense and one can have nothing but admiration for the thoroughness with which it is now carrying out its task. It has received substantial financial help from various American sources (1 695 000 dollars in its first two years alone) and many distinguished physicists have been very closely associated with it. For these reasons alone, the Committee's achievements do deserve very careful appraisal.

The intention is to develop a course complete in itself and at the present time it is very much a course in the making. It is a little difficult to assess fairly what its impact will eventually be until the structure is nearer completion.

But even in its present form, it is impressive and for us in this country it will certainly be a most interesting and stimulating experiment. We will consider later whether or not it can be put to use in this country.

1. The content of the course

In deciding on the subject matter to be included, the Committee was anxious to emphasise the unity of physics, to show it cannot be divided up into independent topics as is suggested by the traditional textbooks. It has tried to stress this unity by presenting topics directed towards a single goal: the atomic structure of the universe. This however has been interpreted in the broadest sense.

Deliberately the course caters for pupils with a wide range of interests, but with no previous knowledge of physics. Technology as such is omitted throughout. Some chemistry has been included in order to make the course an intelligible whole.

The course, which is intended to last one year is divided into four sections as follows:

I. *The universe*

The nature of physics.
Time. Space and measurement. The universe.
Mathematical functions. Scaling. Kinematics. Vectors.
Mass. Atomic structure of matter. Atoms and molecules.

The emphasis is on general concepts rather than on specific definitions and the section gives a general introduction to subject matter which will be studied later in the course in greater depth. There is no hesitation in bringing advanced topics into the text: radioactivity, spectroscopic analysis, x-ray spectra, organic molecules, for example, are all discussed.

II. *Optics and waves*

Rectilinear propagation. Reflection and refraction. Lenses and optical instruments.
Corpuscular and wave theories of light. Wave phenomena. Mechanical waves.
Interference. Thin films.
Diffraction at a single slit. Diffraction phenomena.

This section is intended to show " the coherence and power of physical ideas ". The section certainly succeeds in giving a thorough account of the development of optical theory. At first phenomena are described in terms of rays, the corpuscular theory is developed and when this fails, the wave theory is introduced. Other forms of wave motion are considered. The section concludes with a consideration of diffraction phenomena, which would be considered advanced by English standards.

III. *Mechanics*

Newton's laws of motion.
Motion at the Earth's surface.
Projectiles.
Circular motion. Simple harmonic motion.
Universal gravitation and the solar system.
Momentum. Conservation of momentum.
Work. Kinetic energy. Potential energy. Conservation of energy.

Kinetic theory. Temperature and molecular energy.
Mechanical equivalent of heat.
Heat flow. Energy dissipation and temperature rise.

The conservation laws form a substantial part of this section and lead naturally to a development of the kinetic theory of heat as an application of dynamics in a particular branch of physics.

IV. *Electricity and the atom*

Qualitative electrostatics.
The electric current. Conductivity of solutions and gases.
Coulomb's law. Electric field. Electric potential.
Electric current. Electric power.
Magnetic field. Field due to currents.
Forces on currents in magnetic fields. Meters. Motors.
Electromagnetic induction.
Electromagnetic radiation.
Nuclear model of atom. Scattering of alpha-particles.
Photoelectric effect. Photons.
Wave nature of matter.
Quantum states. Energy levels.
Wave theory of hydrogen energy levels.

The section includes a careful introduction to electric and magnetic phenomena leading on to concepts which are traditionally left to Universities in this country. The photoelectric effect leads to the quantum theory, bringing out the dual nature of light. The diffraction of electrons leads to the dual nature of matter. And so the course returns to the study of the atom.

2. TEXTBOOK

In the first instance, the textbook was printed in separate volumes corresponding to the sections in the course; ultimately they will all be bound in a single volume.

A revised version of Volume I has already been printed and this includes substantial modifications of the original. The revision has been carried out as a result of the experience of the limited number of teachers who used the course during 1957–58. It is intended to make similar revisions of all the volumes. This ' feedback ' process will of course be most valuable, ensuring that the final textbook will be satisfactory from a teaching as well as an academic point of view. This is particularly useful as the course was originally designed by research physicists and university lecturers and only subsequently were teachers brought in: the ' feedback ' will see that the course in its final form is not unaware of the difficulties of the classroom.

The quality of writing is such that the text is easy to read and interesting, though there is a tendency to long windedness, which may be due to the text being the work of a team. (Naturally the text contains many American expressions, which might be a little out of place in our own classrooms, were the books ever used here.) The text is definitely fuller than a comparable textbook in this country. The quality of the illustrations is excellent. Also included are a large number of examples, a major proportion quantitative. The amount of mathematics in the text itself has been kept to the minimum, the full detailed working being left either to the teacher or to the pupil: at least this enables the wood to be seen for the trees.

There is some inconsistency at the moment in the amount of mathematics required. In Volume III some sophisticated mathematics is included and it is assumed that

$$a = \lim_{\Delta t \to 0} \frac{\Delta v}{\Delta t}$$

is understandable, whereas in Volume II, when referring to Snell's law, we are told we may use " what are known as the sines of the angles ". But it is clear that the committee is aware of this and doubtless will modify later versions.

The Committee has obviously omitted a lot of basic physics traditionally found in school courses. This was apparently done with some reluctance, but for them the over-riding consideration was that the course must be completed in one year. It should however be emphasised that less has been left out than may at first sight appear to be the case, and a number of these gaps have been filled by the monographs, which are an essential part of the course as a whole.

3. MONOGRAPHS

The intention is that these monographs will supplement the course by enlarging on topics mentioned in it and also present those topics which have been omitted altogether from it. For example, one of the traditional topics omitted from the course is the physics of sound: three of the monographs planned will fill this gap, dealing with the physics of music, underwater acoustics and ultrasonics.

The monographs will consist of 20 000 to 40 000 words to be issued in a ' quality paperback ' edition to sell " in the range of 65–95 cents ", and it promises to be a full scale publishing venture.

Already some 48 monographs are being written with such varied titles as *Crystals*, *The Neutron Story* (which was reviewed by Sir James Chadwick in the December issue of this journal), *How Old is the Earth*?, *Galileo and his World*, *Magnets*. Classics such as Boys' *Soap Bubbles* are being reissued. It is likely that this series will be a remarkable asset to our teaching in this country and it is good to learn they are to be published in this country by Messrs. Heinemann.

" The monographs will enable students in whom a desire for additional knowledge has been stimulated by classroom and laboratory experiences, to pursue their studies and experiments independently to the limit of their capacity ". This is good teaching: it takes individual differences into account and provides for them.

4. FILMS AND FILMSTRIPS

The films are intended to supplement the course in much the same way as the monographs. About 60 films are planned, lasting about 15–20 min, and being directly related to the teaching cannot fail to be a useful aid. Some of those already complete are *Crystals*, *Short Time Intervals*, *Electrostatics*, *Behaviour of Gases*, *Inertia*, *Forces*, *Change of Scale*, *Pressure of Light*.

The writer has not seen any of the films himself, but the notes that go out with them are of high quality containing a large number of ' stills ' and testify to the usefulness of the series.

5. LABORATORY MANUALS AND APPARATUS

The laboratory manuals contain just under fifty experiments; these range from the very elementary to the most sophisticated, but almost all could be done with very simple apparatus such as is already available in most schools, or could easily be made, and there would be little need here for the kits of apparatus being made available in America.

Many of the experiments are original and the guides to teachers would certainly be of considerable interest to science masters in this country.

6. APPLICABILITY TO USE IN THIS COUNTRY

The course is intended for American schools and they have certain difficulties to be met which are peculiar to their system of education and which are not experienced over here. Their system is loosely knit without the general standard that is set in this country by the General Certificate of Education at all its levels. Secondly, their system tends to require courses to be complete in themselves and often, as in this case, only of one year in duration. Thirdly, American courses do not have to go to the degree of specialization that is, rightly or wrongly, forced on secondary schools in this country by our University entrance requirements.

In this country with our elaborate G.C.E. exams, the need for this course is not great. Certainly no one would accept a course of one year's duration, however elaborately and carefully devised, as a substitute for our present Physics courses, which extend over many years, and of course it is most unlikely our Universities will modify very substantially the entry requirements for their various faculties.

We are well aware, as the Committee began by pointing out, that the present science syllabus in schools dates back half a century and that attempts to modify it have merely resulted in a " patchwork quality ". Our present ' O ' level course is a traditional one, thought of more as a basis for 'A' level rather than as an end in itself, despite the fact that the large majority of pupils taking ' O ' level physics will never go further in the subject. Steps are now being taken to remedy this: the Science Masters' Association has set up panels to examine all the science syllabuses and the Physics panel will be issuing a suggested syllabus that certainly will not " date back half a century ".

Could the P.S.S.C. scheme be used in this country? As a course for study, it is more comparable with an 'A' level rather than an ' O ' level course in the intellectual demands it makes on a boy. Its factual content, however, omits so much traditional physics in order to condense it into one year, that it would be quite impracticable for a pupil to use for any of our present examinations.

As a course for an Arts student wishing to learn in one year something of the nature of science, it could have exciting possibilities, and in any event teachers of science are likely to welcome the ancillary services of the monographs and the films if and when they become available.

BOOK REVIEWS

Handbook of Scientific Instruments and Apparatus. Published in connection with The Physical Society's 44th Annual Exhibition, 18 to 22 January 1960. (London: The Physical Society.) 6*s.*

Punch, in the days when it was like what it used to be, had a feature " Entertainments at which we have Never Assisted "; William Wordsworth wrote a calypso on Yarrow Unvisited. But neither these shining precedents, nor our own inclination towards contemporary slickness, can tempt us into anticipating the contributed report of the Exhibition. This is simply a review of the Handbook, as a publication to be studied. Some exhibitors indeed, have spread themselves in exposition, and there are long and instructive articles on the fabrication and uses of metal tubes, on electronic instrumentation, and on ultra-high vacua.

The exhibits really group themselves under three headings:

1. *Instruments:* These include:

   Digital voltmeters—analogue digital converters, which present the reading in figures, or print it out.

   Mercury vapour monitors for testing the concentration of mercury vapour in the air, using the strong absorption of the resonance line 2537 A.

   Dew-point hygrometers using Peltier cooling in a bismuth telluride couple.

   Transducers of various types, for measuring displacements and accelerations.

   Microminiature circuits of two types: that of the R.R.E., Malvern, using conventional components made on a very small scale, and that of Texas Instruments, in which the whole circuit is built into a single semiconducting crystal, the pattern on and within it being shaped by diffusion, metallic evaporation, alloying, and chemical forming—giving a multivibrator circuit within a volume of a few cubic millimetres.

   Ferromagnetic films, about 1000 A thick, for use as storage elements; as only a small area of film is used for each, the whole can be stacked directly on to a printed circuit, without any wiring; further, the anisotropic material permalloy, magnetizable parallel and perpendicular to the axis, can store information in binary form. Several examples of the use of the moiré fringe technique; an intensity-correlation interferometer of the Hanbury Brown-Twiss type for measuring stellar diameters; and systems for recording and processing information from satellites are also exhibited.

2. *New materials:* These include:

   Silicon nitride, a refractory that withstands thermal shock, for the nozzles of rockets, blades of gas turbines, and containers for atomic fuels; *gallium arsenide,* an intermetallic semiconductor with very high electron mobility; piezoelectric *lead-titanate* (*zinc*) ceramic; electroluminescent dielectrics, both organic and inorganic; " square-loop " materials for memory elements; and, on the larger scale, microwave absorbing materials for the radar " dark room ".

3. *Instructional exhibits*

   From the Science Museum, models of a cyclotron and a cascade generator; from U.K.A.E.A., models illustrating the principles of the direct conversion from heat to electrical energy; a supersonic wind tunnel adapted for schlieren projection; and contributions from University and Hospital physics departments.

The value of this Handbook is enhanced by its early publication well in advance, which will have allowed the visitor to do some selection and possibly some homework beforehand. It will be preserved afterwards as a work of reference on the most recent developments in scientific equipment.

G. R. NOAKES.

15 *to* 18. *A Report of the Central Advisory Council for Education—England.* The Ministry of Education. (London: H.M.S.O., 1959.) 12*s.* 6*d.*

The 1944 Education Act spoke of the compulsory raising of the school-leaving age to 16, with part-time education continuing to 18. Its implementation is recommended in this Report as a matter of national urgency, the first of these steps being taken within the period 1966–69. Ill fares the land where wealth accumulates and men decay, especially if they start in their teens; that is the tenor of this, the Crowther Report. Chief concern is for children who, leaving school at 15, make no further contact with education—about half the total of young people in the country; not generally of markedly inferior intelligence to those who reach the sixth forms of grammar schools. This ' neglected educational territory ' must be called upon to furnish the manpower needed for the new technological age. Those who pursue part-time courses in further education need exceptional staying-power under present conditions, and the wastage and length of time involved are at present far too great.

The reason for the choice of dates is that this represents a minimum in the teen-age population curve. In five years' time there will be 700 000 more in this age-group than at present, and in ten years' time only 400 000 more. The next five years will be a time of intense pressure; if the system can stand the strain of that (and in five years many schools can be built, and many teachers trained) it should be ready for developments in the breathing space. That is, if resolute advantage is taken of this opportunity, which may never recur.

Compulsory raising of the leaving-age to 16; development of county colleges for part-time education up to 18, reinforced by a strong Youth Service; more efficient organization of further education, with block-release rather than day-release; sandwich courses (college-based) rather than part-time courses; and co-operation between further education institutions and schools to ease the transfer to the new stage—all these are among the suggested requirements.

Part Five of the Report, seven chapters in all, covers the sixth-form work of grammar schools, dealing with the curriculum, the treatment of science, staffing, examinations, and university entrance regulations. Specialization as we know it today is approved, though the offering of more than two subjects at Advanced Level in general is not; the effective use of ' minority time ' is discussed. This is a section on its own in effect, and it is hoped to comment more fully on it in another article; the problem of university entrance itself could be taken up at length, and science syllabuses would have to be.

Chapter 37 deals with teachers, the qualities required in the profession (chastening reading indeed at the end of a strenuous term!) and the problems of recruitment. Short-service engagements and other emergency measures are suggested, and an energetic campaign of recruitment, particularly at the universities. The novel prospect of at last actually getting the people who *really could* influence the situation to do so dumbfounds one, and provokes some silent meditation. What, says the Report, can such a campaign offer to attract? Well, has anyone ever analyzed the factors that make a man decide on his career at all? One can only say that science masters, individually and collectively, appear to be a happy race of men, regarding themselves as fortunate in doing what they enjoy; it is suggested that, seen from the customer's side, the teacher's lot looks unattractive, but I am not so sure. Certainly teachers themselves, and university education departments, could do some useful priming work at the sixth-form stage. And, if every Director of Education's job depended on him seeing that all his schools were fully staffed they would be, somehow. In this matter, there are some psychological factors to consider. First, for the graduate at any rate, the talent-spotting by industrial firms and the prestige that surrounds a Career in Industry as against a mere teaching job; we ought to sell a Career of Service in Education as against a job in a soap-works. The Report mentions the public's low regard for the profession; public regard is an artefact nowadays, and there are means available for redressing this. No deliberate cultivation of regard is noticeable in the mass-media, some of which see in the educated man a critic, and in the educator an enemy, and have their own subtle defence mechanisms. But they are run by citizens and parents, and could be won over.

This Report should be read and studied by everyone concerned with education; its forthrightness (in places) should be a model for all who have to negotiate with the Administration. It concludes with the emphatic statement that the primary need for implementing the proposed programme is a public determination to will it. Does this, one wonders, exist? When this is forthcoming, the next step is a research programme to ascertain facts; there are, it seems, the most extraordinary gaps in our knowledge of what goes on in the schools and technical colleges that we have today, let alone in the minds of their pupils. Some people have said that the Report is too trenchant, and tried to play it down; but the sense of urgency throughout it is all too well-founded.

Chance and Providence. By WILLIAM G. POLLARD. (London: Faber and Faber Ltd., 1959.) [Pp. 192.] 16*s.*

Perfecting the Newtonian system of mechanics, Laplace bequeathed to posterity the concept of a physical universe, completely closed, completely determinist, and completely predictable. The whole structure of classical physics was erected upon this foundation, which showed no sign of cracking until the beginning of the present century. So well was the work done that the ' classical façade ' is still taken for granted on many sides as the authentic doctrine of modern scientific man. To the Christian nurtured on the Scriptural doctrine of God, and indeed to the humanist with a deep understanding of the achievement of the human spirit in other fields, the universe appears to be something far more open, more rich, and less circumscribed. Where one and the same person has imbibed both the scientific and the Judeo-Christian tradition, the resulting intellectual tension can become intolerable. The author of *Chance and Providence* has known precisely this experience; and his professed aim in writing is to clarify the issue for those who share his experience of intellectual tension.

The key to the situation is to recognise that under the actual conditions obtaining in the world of nature, as distinct from the highly artificial conditions commonly imposed in the laboratory, not one but a plurality of possible causal sequences may be visualised at any given moment. In principle a mathematical probability may be assigned to each; complete predictability, however, is an impossibility in the actual universe. The indeterminacy postulated by quantum mechanics is seen as a particular instance of this truth on the microscopic scale of events. But events on the large scale, taking place under conditions in which unknowable disturbances cannot possibly be excluded, afford numerous instances of the same openness, and similarly throw us back on to statistical formulations of natural law.

The author sees every particular moment in the history of the universe as a situation which can be known and interpreted with equal validity in either of two ways: in terms of chance, or in terms of providence. Until an event has actually taken place, the possibilities of the situation can be validly expressed in terms of probabilities, which may be vanishingly small, or overwhelmingly large. Science can do no more than state the probabilities; and to do so is a valid insight into the situation appropriate to the scientific approach to reality. On the other hand, someone standing within the Judeo-Christian tradition, viewing the same events after they have happened, and apprehending something of their historical consequence and meaning, may recognise the guidance of providence in the ' choice ' which was made from among the possibilities of the situation. This equally is a valid insight.

To the reader whose approach is purely and narrowly scientific, the insight which discerns the working of providence may appear to fall into the error of subjectivity. The author can hardly be unaware of this possible objection. In fact by devoting a section of his book to Martin Buber's well-known contrast between the ' I-it ' and ' I-thou ' approaches to reality, he is in effect claiming that a measure of subjectivity (in this sense) is unavoidable if one is to come to terms with human experience in all its richness.

This is indeed a remarkable book, which will repay careful attention. Not less remarkable is the author himself, who is at one and the same time an atomic physicist of distinction and a priest of the Anglican Communion—a two-fold qualification which gives him the authority to write on this baffling subject. As one who has wrestled with a real problem, avoiding slick solutions, and holding fast to both aspects of truth as he has learned it in his scientific and in his spiritual life, the author has a right to be heard. While not all would agree that the last word has been said, most readers will feel genuinely grateful for a courageous attempt to resolve an old tension, and to bring wholeness into the modern scientific man's approach to the things of the spirit.

G. D. YARNOLD.

The Advancement of Science. Vol. XVI, No. 62. (London: The British Association for the Advancement of Science, 1959.) [Pp. 154.] 7*s.* 6*d.*

This comprises the Presidential address, *The Proper Study of Mankind is Man*, delivered at the York meeting in September 1959 by Sir James Gray, and the addresses by the presidents of individual sections, viz. *The Visualization of Magnetic Processes* by Professor L. F. Bates; *Medical Aspects of Carbohydrates* by Professor M. Stacey; *Recent Developments and Trends in Palaeontology* by Professor O. M. B. Bulman; *Man and the World's Fauna* by Dr. L. H. Matthews; *Trends in Urban Expansion*, by Professor K. C. Edwards; *How Much Science?* by Professor J. Jewkes; *The Critical Importance of Transport and Communications*, by Sir Ewart Smith; *The Nature and Scope of Archaeology*, by Professor I. A. Richmond; *Artificial Organs: Biological Applications*, by Professor A. Hemingway; *Perception, Attention, and Consciousness*, by Professor M. D. Vernon; *Plants on Land and in the Oceans*, by Dr. W. R. G. Atkins; *What are Our Schools For?* by Sir J. J. Robertson, and *Balance in British Farming* by Dr. H. G. Sanders.

Professor Bates' address showed the developments of the modern Bitter-pattern process for following the motions of domain boundaries, and the new knowledge made available by the plastic-and-electron-micrograph process; and also the use of the Faraday effect in a new optical-reflection technique. He explained the picture that is gradually being built up of the conditions at domain boundaries, and what goes on during the magnetization of a ferromagnetic.

Sir James Gray, in the concluding section of his address, spoke of the need for a wider outlook in the teaching of science and, for the scientist, either a more general relation of science teaching to other disciplines, or else the widening of his interests by formal teachng in other fields. Sir James Robertson's thesis was the sordid material end, the ineffective means, the poverty of resources, and the general public apathy which characterize the " lowly place and function a plutocratic society gives to education ". (The Crowther Report endorses much of what he said.) Professor Jewkes critiziced some of the more familiar reasons for the statement that this country is suffering from a shortage of scientists and technologists, and suggested that we should continue to search for better criteria for assessing the need.

The Diesel Engine. By L. V. Armstrong and J. B. Hartman. (New York: The Macmillan Co., 1959.) [Pp. viii + 360.] 45*s.* 6*d.*

The authors' avowed aim is to provide a comprehensive picture of the diesel engine. That this cannot be done in a volume of this size is a measure of the range of subjects to be covered. A general introduction and chapter on engine cycle thermodynamics is followed by sections on Combustion, Dynamics, Statics, Parasitics and Economics. Final chapters in each of the first three sections are devoted to a ' special project ' involving the selection of a design for a specific duty, its economics forming the main topic of the final section. This is an unusual topic for an engineering textbook and stimulating on that account.

The reader, however, needs at least an elementary grounding in thermodynamics and a rough working knowledge of the mechanism of an internal combustion engine. Emphasis throughout is on the large, stationary engine and American practice is dealt with to the almost complete exclusion of even the most significant recent European developments. Within these limits the book is useful, but stress is on the ' how ' rather than the ' why ' of the technologies covered. The difficulties of adaptation of the diesel to the variable-load, variable-speed conditions required in land transport applications are acknowledged, but no adequate analysis or discussion of these problems is given. Some assessment of the relative economies and limitations of the diesel engine as compared with other prime movers would have clarified the picture presented in many ways. The section on Combustion leaves a somewhat confused impression, and a change in order of the chapters would have improved comprehension by the student, even though the (disputable) logic of the sequence would have been disturbed. It is odd to find exhaust turbocharging discussed under the general heading ' Parasitics ', when few marine 4-stroke engines are without it, and its use is even invading the road transport field. This is an uneven book, and typical, perhaps, is the relegation to the end cover of the more useful of the two thermodynamic charts included, where it becomes virtually useless. This is unworthy of an otherwise well-produced volume.

J. Spiers.

A history of neutrons and nuclei. Part 2

by N. FEATHER
Department of Natural Philosophy, University of Edinburgh

5. A FOOL EXPERIMENT DISCLOSES THE REAL NUCLEUS

As we have seen, early in 1909 Rutherford put ' Young Marsden ' to work, under Geiger's direction, on a ' fool experiment ', a practical essay in speculation. The object of the experiment was to see whether there might be any ' diffuse reflection ' of α-particles from metal foils. Incredibly, as it seemed, the effect was there, small in intensity, but incontrovertibly genuine. Rutherford pondered over it for eighteen months before he committed himself to an explanation. In 1906 he had found that a beam of α-particles passing through a foil is slightly spread out due to ' scattering ' in the foil. When Marsden was given his problem, Geiger had already examined this phenomenon in some detail, and his work was continuing. During the winter of 1909–10 he obtained more comprehensive results; then it became clear to Rutherford that the two phenomena—of diffuse reflection and small-angle scattering—could not be fitted into a single pattern of explanation on the basis of existing ideas. It was not impossible to explain the detailed results concerning small deflections on the basis of Thomson's model, but the qualitative fact of large deflections, in the circumstances in which these were observed, was sufficient to show that this model was in some sense grossly inadequate.

Here, for historical accuracy, it is necessary to digress briefly to record what, at this distance, appears a rather remarkable passage in a long paper by W. H. Bragg published in the *Philosophical Magazine* of September 1910. The paper is undated, but it must have been written during the early summer of that year, at a time when Rutherford was still groping towards an explanation of the α-particle effects. Bragg was discussing the scattering of β-particles (in relation to his neutral-doublet hypothesis, which we have considered already), and was attempting to show that the Thomson atom—or, at least, the theory of β-particle scattering which Thomson had based on his atom model—failed to provide a satisfactory explanation of this scattering. He wrote: "Actually there is no such state; however thin the plate the highly scattered rays are in a certain proportion to the slightly scattered rays . . . . From the very first large deflexions must be considered. The many slight deflexions which the β particle experiences . . . are of no real consequence; little more than in the case of the α particle ". It is the last phrase that is so remarkable in its context, but Bragg did not elaborate. Moreover, it was abundantly clear that he had no notion of a nuclear atom, for elsewhere in the paper he wrote: "An α particle has a considerable speed . . . and as an atom of helium it must contain several electrons . . .".

Rutherford finally thought his way through this particular problem in the latter half of December 1910. On 7 March 1911 he made the first public announcement, at a meeting of the Manchester Literary and Philosophical Society; during the next month he wrote out his ideas in full, and the resulting paper, ' The scattering of α and β particles by matter and the structure of the atom ', duly appeared in the May number of the *Philosophical Magazine*. In a brief introductory section he stated directly: " The theory of Sir J. J. Thomson . . . does not admit of a very large deflexion of an α particle in traversing a single atom, unless it be supposed that the diameter of the sphere of positive electricity is minute compared with the diameter of the sphere of influence of the atom "; then he went on: " We shall first examine theoretically the single encounters with an atom of simple structure, which is able to produce large deflexions . . .".

That was the order of presentation in the paper, but there is at least a doubt whether that was in fact the order in which, once the general notion had become clear in his mind, Rutherford followed out its detailed consequences. There is in existence a sheet of calculations (see Robinson 1943), undated and in Rutherford's hand, headed ' Theory of structure of atom '. The writing begins: " Suppose atom consists of +charge *ne* at centre and −charge as electrons distributed throughout sphere of radius *b* ". This is very closely the wording by which the calculation of large deflections is opened in the published paper (though the central charge is there $\pm Ne$, Rutherford having convinced himself that, so far as large deflections were concerned, a negatively charged nucleus would be as effective as one positively charged, and, to begin with, no mention is made of electrons, rather of " a charge $\mp Ne$ supposed uniformly distributed throughout a sphere of radius *R* ")—but in the manuscript sheet the calculation is of small deflections. It appears very likely that Rutherford's first concern regarding his new model was that it should be equally as successful as Thomson's in representing the phenomenon of small deflections. Satisfied upon this point, he proceeded to work out its implications in relation to single scattering at large angles. When he came to discuss the small-angle scattering in the paper he overplayed his hand, and attempted to show that " the magnitude of the diffuse reflexion . . . and the magnitude of the average small angle scattering . . . are both explained on the hypothesis of single scattering by supposing that the atom . . . has [the same] central charge . . .". Later he withdrew this claim (*Radiations from Radioactive Substances*, p. 209, 1930); he wrote: " The experiments on multiple scattering have indeed led to no definite conclusions about atomic structure and, speaking generally, the best that can be done is to show that the results are in accord with the nuclear theory. . . . The first experiments of this kind were those of Geiger . . .".

There are two points in the general discussion with which Rutherford concluded his 1911 paper which are worthy of note as showing his caution, at least in print, at that time. He wrote: " Considering the evidence as a whole, it seems simplest to suppose that the atom contains a central charge distributed through a very small volume . . . . At the same time, the experimental evidence is not precise enough to negative the possibility that a small fraction of the positive charge may be carried by satellites extending some distance from the centre ". This last remark was, in all probability, a concession to Bragg, who had written on 5 January 1911: " I think you will have to put some sub-centres into your atom to explain the x-ray effects ". Then again, Rutherford wrote: " The deductions of the theory so far considered are independent of the sign of the central charge, and it has not so far been found possible to obtain definite evidence to determine whether it be positive or negative ". So he preserved an open mind, at least in public, on these two points.

Granted the validity of the inverse square law of electrostatics for the particles within the atom, the necessity of assuming a massive charged nucleus was convincingly demonstrated by Rutherford's analysis: it was not until April 1913, however, that the detailed experiments of Geiger and Marsden effectively removed the lingering doubt which that proviso implies. The precise confirmation of theoretical prediction which they then achieved could mean little else than that the electrostatic law is in fact valid down to the minute distances involved. Thomson, however, had imagined otherwise: " This seems to me to indicate that the large deflections . . . are not produced by forces . . . due to electrical charges . . . which would act upon a charged corpuscule as well as upon a charged α particle. They are in my opinion more likely to be due to special forces which come into play when two α particles are within less than a certain distance of each other. That in fact when two α particles come into collision inside an atom the forces between them are not merely [electrostatic] ". In spite of the fact that events proved its lack of relevance to the problem of the time (1913), this thought of Thomson's has a very modern cast to it.

In the introduction to this article, it is asserted: " Since 1913 . . . this word [nucleus] has been used without ambiguity . . .". Our history has now reached that point in time: for present purposes, therefore, our story of the vicissitudes of the nuclear atom model is complete. In what follows it is necessary only to complete the history of the concept of a neutron as a nuclear constituent particle.

On 19 March 1914 the Royal Society organized a discussion on the structure of the atom. Rutherford said: " The [nucleus] must consequently be considered as a very complex structure . . . consisting of positively charged particles and electrons, but it is premature (and would serve no useful purpose) to discuss at the present time the possible structure of the nucleus itself ". In assessing this remark, two inferences can be made with fair certainty: first, that Rutherford had already given considerable thought to the problem, secondly, that others less close to the experiments on which alone depended further advance—and less close, he may have thought, to the source of inspiration—had been putting forward their speculative views. Ramsay had written *Elements and Electrons* in 1912, and, at the Solvay Conference in 1913, Thomson had had his say: " The fact . . . suggests that the atoms of the elements of even valency have for their positive nuclei aggregations of various numbers of helium atoms, while those of the atoms of odd valency are aggregations of various numbers of helium atoms together with three hydrogen atoms. For the elements whose atomic weight is greater than 40, the arithmetical relations . . . cease to be as orderly . . . a more fundamental break . . . occurs in the law of structure of the atom ". Possibly this remark served no strictly useful purpose at the time, but in retrospect it may be admitted that there was, after all, sense in it.

From the Royal Society discussion in 1914 Rutherford went forward to Washington to deliver the first William Ellery Hale lectures. In these lectures he gave some inkling of those thoughts which it might have been premature to have disclosed on the previous occasion. The sub-title of his lectures was ' The evolution of the elements '. It was in this connection that the possibility of probing the structure of the nucleus seemed to him worthy of consideration. " Sir Norman Lockyer and others have suggested that the elements composing the star are in a state of inorganic evolution ", he said. To accept this suggestion was to imply that in the stars atomic nuclei are being built up—and possibly broken down—on a vast scale. " It is possible that the nucleus of an atom may be altered either by direct collision of the nucleus with very swift electrons or atoms of helium such as are ejected from radioactive matter . . . . Very penetrating X-rays or gamma rays . . . may prove to be possible agencies for changing atoms ".

Here, when Rutherford referred to ' altering ' and ' changing ' atoms, he had one eye on the stars and the other on the laboratory. He must certainly have known that the astronomer's guess would be that the temperature in the interior of a star is far below that which is necessary (some 10^{10} deg. C) to ensure that a significant fraction of the atoms have kinetic energy as great as that of the α-particles from radium. For him such knowledge must have pointed in two directions. If, indeed, nuclear transformations due to charged particle collisions are effective on a large scale in the stars, then it must be possible to detect them on a small scale in the laboratory; conversely, if stellar temperatures are not high enough for such collisions to be effective, then collisions of some other type must be effective instead. So, we may suppose, Rutherford considered the possibility of transmutation by X-rays or γ-rays. These, at least, are uncharged entities. On the other hand, to regard them as the main agency of transmutation could not have been entirely satisfactory, for it restricted the evolutionary process to one of disintegration, excluding the possibly more fundamental process of synthesis.

The Washington lectures bring us almost to the outbreak of war in August 1914; if we do not take up our history again until the summer of 1919, we shall not have missed much of immediate importance. True, in November 1917, Rutherford gathered the first-fruits of his cosmical speculations, observing the hydrogen nuclei produced in nitrogen under α-particle bombardment, single-handed save for Kay, the laboratory steward. But the results were not published until June 1919.

By the summer of 1920 the situation was very different. Aston's mass spectrograph had begun to function smoothly, and, one after another, the elements were yielding to his analysis. For the first time, those who were inclined to speculate about nuclei knew, in some measure, what the facts were which they had to respect—if not to explain. An American chemist, W. D. Harkins, had been speculating openly, without the benefit of this knowledge, since 1915. In a long paper in the *Physical Review*, in February 1920, he first took note of Aston's early results. He suggested that a combination of two hydrogen nuclei and two electrons, to which he gave the name ' μ particle ' might be an important constituent of heavier nuclei. He wrote: " If this μ particle exists alone it forms the atoms of an element of zero atomic number." Later in the same year, in the *Journal of the American Chemical Society*, he went further: " atoms of zero atomic number may exist and be of importance in atom building. Such atoms might have masses 4, 3, 2 and 1, and possibly other values . . .". Moreover, it appears that he had some appreciation of the ' useful ' properties of such hypothetical particles: "As the total net positive charge on the nucleus . . . increases, the repulsion due to this cause may . . . increase to such an extent that α particles are no longer able to pass through the region of repulsion, and to attach themselves, though electrically neutral particles . . . could easily pass into and through this region ".

Harkins continued to elaborate these ideas in two papers published in 1921 and in another which appeared in the following year. In the first of these papers he used the word ‘ neutron ’ for the first time. At the other side of the world, in Melbourne, Australia, another chemist, Masson, had been engaged in a similar, though less diffuse, essay in systematisation. Towards the end of 1920 he sent a draft paper to Rutherford for criticism. The paper appeared in the *Philosophical Magazine* of February 1921. Rutherford contributed a footnote, in which he wrote: “ Professor Orme Masson sent the present paper for publication through the writer, and in order to avoid the long delay involved in correspondence, the paper is printed in its original form . . . a somewhat similar type of nomenclature for the constituents of atoms has been suggested in the interesting paper of Professor W. D. Harkins.” Masson, in fact, had noted the significance of the parameter to which, nearly a year later, Harkins gave the name ‘ isotopic number ’—a name still used today for the difference between the numbers of neutrons and protons in any nucleus—and in consequence had been led to postulate a singly charged particle of mass 2 “ as a secondary unit of positive charge ”, also “ electrically neutral couplets, each having unit mass ”.

The receipt of Masson’s paper had been somewhat embarrassing for Rutherford, but the chance of writing a footnote to it gave him an opportunity of referring to the parallel work of Harkins which he did not miss. The embarrassment arose from the fact that Rutherford had already given an account of his own speculations in the course of his Bakerian lecture on 3 June, and the lecture had appeared in full in the *Proceedings of the Royal Society* in July 1920. He did not consider that Masson had gone any farther in respect of nuclear systematics, or as far in respect of the essential physics of the problem as he himself had gone in the lecture, though it was clear that his approach had been an independent one. Again, Rutherford had made no mention of Harkins’s μ particles in his lecture, though Harkins’s suggestion had been in print several months when the lecture was delivered.

In the upshot, the decision to send forward Masson’s paper for publication ‘ in its original form ’ had hardly been taken when Rutherford received a letter from Harkins himself. Harkins had now read the Bakerian lecture, and he wrote: “ I was very much interested to note that you consider the possibility of the existence of an element of zero atomic number, especially since I made this suggestion in a paper sent to the *Physical Review* more than a year ago.” Possibly when Harkins read Rutherford’s footnote to Masson’s paper he was just a little mollified, at least for a little time. Rutherford, on the other hand, had he expressed himself openly, might well have invoked “ a plague on both your houses ”: certainly, two months later, writing to an old friend and colleague at Yale, he was uninhibited—and essentially realistic: “Actually, however, most of the ideas . . . have been common property in this country and especially to myself for the last five years. It is exceedingly easy to write about these matters, but exceedingly difficult to get experimental evidence . . .”.

It is precisely in the context of these remarks in the letter to Boltwood that Rutherford’s pre-eminence amongst the neutron-makers of the early 1920’s stands out most clearly. He not only ‘ wrote about these matters ’, albeit with restraint, but he also essayed the much more difficult and adventurous task of looking for the ‘ experimental evidence ’. What he said in the Bakerian lecture has often been quoted: “ it seems very likely that one electron can also bind two H nuclei and possibly also one H nucleus. In the one case, this entails the possible existence of an atom of mass nearly 2 carrying one charge, which is to be regarded as an isotope of hydrogen. In the other case, it involves the idea of the possible existence of an atom of mass 1 which has zero nucleus charge . . . . Such an atom would have very novel properties . . . it should be able to move freely through matter . . . it should enter readily into the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field. . . . The existence of such atoms seems almost necessary to explain the building up of the nuclei of heavy elements . . .". What Rutherford did by way of initiating an immediate experimental search for his neutron has received less frequent mention. It is described in two papers, the former by Glasson (1921) and the latter by Roberts (1922). In each case an attempt was made to obtain evidence for the production of neutrons by the combination of protons and electrons in a hydrogen discharge. In each case the attempt failed: no such evidence was found. For our present purposes, Glasson's paper is the more interesting of the two, though his conclusions were less specific than Roberts's. Glasson prefaced the account of his experiment with an expanded version of Rutherford's views on the significance of neutrons in the cosmological process. Glasson wrote: "This building up process is apparently at work in the evolution of stellar systems from the nebular state. It is, however, difficult to see how an additional positively charged hydrogen or helium nucleus could penetrate into the nucleus of a heavy atom . . . . On the other hand, the entrance of a neutral particle would be much more easily effected, and the subsequent expulsion of the negative electron would achieve the necessary increment of nuclear charge." It is to be wondered whether Fermi, in 1934, knew that his own discovery of the artificial radioactivity following neutron capture had been so nearly foreseen thirteen years earlier.

6. A CHANCE OBSERVATION DISCLOSES THE REAL NEUTRON

So the neutron was not discovered in 1922, but Rutherford retained his conviction that it was no mere fantasy. He referred to the idea again in his Centenary Address to members of the Franklin Institute at Philadelphia in 1924, and it recurred in one aspect or another in papers which he wrote in 1925, 1927 and 1929. For the first and the last of these papers he had Chadwick as collaborator. Then, in 1930, a chance observation was made by Bothe and Becker for which, in retrospect, it may reasonably be claimed that it was the first observation of an effect due to neutrons. It was not so interpreted at the time. These workers had undertaken a systematic search for γ-radiation emitted from light elements under α-particle bombardment. One possibility of explaining the 1929 observations of Rutherford and Chadwick on the protons obtained from the α-particle bombardment of aluminium required that there should be such emission. Bothe and Becker were able to detect a penetrating radiation from aluminium, and there was nothing in their investigation of its penetrating power to suggest that it was other than γ-radiation. They obtained results of a similar nature when boron and magnesium were bombarded, which caused no surprise, for these elements yielded protons also, but they were surprised to find penetrating radiations from beryllium and lithium, which had been examined for proton emission without success. Moreover, the radiation from beryllium was much more intense than that from any of the other elements investigated, and its penetrating power appeared to be greater than that of any known γ-rays. However, Bothe and Becker clearly regarded it as γ-radiation.

During 1931 the penetrating radiation from beryllium was the subject of detailed investigations in both Paris and Cambridge. In Paris, I. Curie confirmed its great penetrating power, finding it less absorbed in her experimental arrangement than Bothe and Becker had done in theirs, and, in Cambridge, Webster found that the radiation emitted in the direction of motion of the bombarding α-particles was more penetrating than that emitted in the opposite direction. This last result in particular was difficult to explain on the assumption that the radiation was γ-radiation, and a brief attempt was made to see whether the expansion chamber might provide a clue to this strange behaviour. Webster had considered the possibility that the effect was caused " not by electromagnetic radiations but by high speed corpuscules consisting, e.g. of a proton and an electron in very close combination ". It was thought, at the time, that such a corpuscule might give rise to a very thin track in the expansion chamber. Champion took photographs of some fifty expansions without noting anything of the kind, and the attempt was abandoned. As subsequent events proved, an opportunity was thereby missed. Webster concluded " it is justifiable, for the present at any rate, to assume the secondary radiations to be electromagnetic in nature. This point is interesting in view of the . . . usefulness of the conception of neutrons in accounting for astrophysical and nuclear phenomena ".

Webster's paper was sent for publication in January 1932, and in the same month Curie and Joliot finished a series of experiments with a thin-windowed ionization chamber to determine whether the beryllium radiation produced disintegration effects in other elements. None was found, but the surprising observation was made that protons of about 4 MeV energy were projected from all hydrogenous materials. Clearly, an elastic collision process of some kind was responsible for this effect: Curie and Joliot could only suggest that it was a Compton-type process in which a high-energy γ-quantum is scattered by a proton.

It is difficult to know how far this perverse view of the matter—perverse because it carried with it so many serious difficulties of a quantitative nature—was personal to the authors, or how far it was adopted by them, uncritically, and for lack of something better. But it should be recorded that Millikan had been in Europe in November 1931, having visited Paris on the 20th and Cambridge on the 23rd of that month. At that time Millikan was a tireless advocate of the view that the primary cosmic radiation consists of γ-rays of high energy. He had with him two expansion chamber photographs then recently taken by Anderson. Three years later he wrote: " I presented these photographs in lectures in both Cambridge, England, and Paris . . . and interpreted [the first] as showing that a cosmic ray [photon] had knocked out of a nucleus a proton . . . and a negative electron. . . . The [second] photograph bothered us very much . . . no trace of a difference in ionization between the right- and left-hand tracks . . . was discernible."

By the time that these words were written the second photograph, if not the first, and many others obtained during the following year (1932), had been correctly interpreted as evidence for the production of positron-electron pairs by γ-radiation of high energy. Moreover, the discovery of this mode of interaction of quanta and atoms had proved once and for all that there is no γ-radiation significantly more penetrating than the ' classical ' γ-rays of thorium active deposit—at least when a heavy element such as lead is used as absorber. If this fact had been known when Bothe and Becker made their first observations on the beryllium radiation in 1930 it would have been sufficient, of itself, to have ruled out of count any explanation of their results in terms of a high-energy quantum radiation and nothing else—nor would anyone, in 1931, have given serious consideration to the view that the primary cosmic radiation consists of photons of high quantum energy.

But the fact of pair production was not known in January 1932, and it was left to Chadwick, in Cambridge, to carry out the crucial experiment which proved beyond reasonable doubt that the quantum hypothesis was not only perverse but untenable. Chadwick allowed the beryllium radiation to pass through an ionisation chamber so arranged that any secondary effects occurring in hydrogen, helium, lithium, beryllium, boron, carbon, nitrogen and argon would in turn be recorded. In each case he was able to detect the nuclei set in motion by elastic collision, and to determine the maximum energy acquired in the process. He showed that these recoil energies could not consistently be explained in terms of a Compton scattering process: they could, however, be explained satisfactorily if the penetrating radiation consisted of neutral particles of unit mass. We need not here pursue this subject farther; Chadwick's interpretation was accepted without demur—as nearly as might be, he had at last discovered the neutron for which Rutherford, and he himself, had been keeping watch for so many years.

The modern theory of the structure of the nucleus had its origin in the experimental discovery of the neutron by Chadwick in 1932. For some years previously, quantum-theoretical physicists had been increasingly aware of the great difficulties in basing a satisfactory theory of nuclear structure on the assumption of protons and electrons as structural units—for one thing, the mass of the electron is too small for it to have individual existence in a nucleus of sharply defined properties. This difficulty, at least, is not in question if all the building units are assumed to be of protonic mass. In 1932, Heisenberg, in particular, was quick to develop the neutron-proton model in theoretical terms. Harkins (1933) reminded the scientific world of his own earlier speculations: " The fundamental nature of this relation [the ratio of number of neutrons to number of protons as significant for nuclear stability] pointed out in 1921 by the writer has just been recognized by Heisenberg . . .", but the scientific world was not greatly impressed. By 1933 the neutron of unit mass was no longer an item of speculation: it was already at least as real as the electron and the proton.

We have just paid tribute, in passing, to one theorist, Heisenberg: we should be lacking impartiality, and be failing to complete our history, if we did not, at this stage, pay equal tribute to another. Wolfgang Pauli spent part of the summer of 1931 attending a theoretical seminar at Ann Arbor, Michigan. One result of his visit was a letter written on 9 October of that year, and duly published in the *Physical Review*, over the signatures of J. F. Carlson and J. R. Oppenheimer. The title of the letter was ' On the range of fast electrons and neutrons '. A footnote stated: " We are much indebted to Dr. Pauli for telling us . . . the elements of the theory of the neutron, its functions and its properties ". Pauli's neutron, however, was not the neutron of Rutherford, which Chadwick was to discover the following year: Carlson and Oppenheimer wrote " It will be remembered that, according to Pauli, it is one of the functions of the neutron to carry off the apparently lost energy in a radioactive beta-ray disintegration ". But that—according to Pauli—was not to be the only function of the hitherto unknown particle which he was postulating, as will shortly appear.

Carlson and Oppenheimer soon followed up their letter with a full-length paper. Just before it was completed, Chadwick's discovery was announced. The American authors were able, therefore, to comment on the changed situation, admitting the obvious, that indeed Chadwick's neutron was not the neutron which Pauli had told them of at the seminar of the previous summer. As we have seen, the former neutron, the neutron of unit mass, eventually provided a natural way out of the first difficulties of the theorist in relation to nuclear structure. A year previously Pauli had envisaged his neutron (of much smaller mass) as providing a way of escape, at least from some of these difficulties: " Pauli supposed that such neutrons might form a third element in the building of nuclei, in addition to the electrons and protons . . .". It did not turn out that way. Pauli's original suggestion was too ambitious, but his intuitive solution of the problem of β-disintegration was essentially sound. In 1933 Fermi re-named the hypothetical particle of Pauli the ' neutrino ', and made it fundamental for his theory of the disintegration process. Thereafter, for over twenty years, the experimental physicist accepted the existence of this elusive particle, the observed properties of which were no more than sufficient to justify its name—until, by 1956, Reines and Cowan, with prodigious effort, were able to adduce slender, but convincing, evidence of effects attributable to neutrinos in free flight. At last the neutrino of Pauli had become equally real with Chadwick's neutron, or very nearly so. In the end we are better informed than Pauli was, if not wiser: we know that at least two neutral particles are required for plausible theories of β-disintegration and the structure of nuclei, not one.

If the original neutron of Pauli has not, in the upshot, all the properties which he supposed it to have, the situation is very little different with the neutron of Rutherford. Rutherford regarded his neutron as a stable combination of proton and electron, more stable in fact than the neutral atom of hydrogen. He thought of it, as we already know, as the most important agent in the process of the building up of heavier nuclei out of lighter constituents, even, according to Glasson, under conditions of extremely low density, in the primordial nebulae. But, in the upshot, the neutron which Chadwick discovered is significantly more massive than the atom of hydrogen; in the free state it is radioactive, breaking up spontaneously into a proton and an electron, having an average life of not much more than quarter of an hour. We now believe that the capture of protons is immensely more important in the cosmic process than is the capture of neutrons. Such are the hazards of hypothesizing; yet without hypothesis science is sterile. The neutron of unit mass is, in many ways, Rutherford's neutron, in spite of it all.

7. Hyperneutrons and hypernuclei

In these days any history of neutrons and nuclei would be incomplete which did not at least refer to the ' strange ' particles of the modern era. First, in minute intensity in the cosmic radiation, and more recently in the radiation field of large man-made accelerators, physicists have been able to study the interactions of particles and nuclei at very high energies. In these interactions energy becomes materialized in a variety of ways. A whole spectrum of new particles of transient existence, and strange properties, results. Some of these particles are charged, some uncharged: the latter, in the literal sense, are neutrons—though it would obviously be unnecessarily confusing to call them by that name for purposes of identification. There are neutral particles of mass some 265, 965, 2180 and 2325 times the mass of the electron—and there may be others. In general the name ' hyperon ' is given to a particle of mass greater than that of the neutron of 1932 (mass roughly 1839 electron masses). In the present context the hyperon of mass 2180 electron masses, the so-called Λ-particle, may be singled out for special mention. In the free state it has a lifetime of about 3×10^{-10} sec, breaking up into a neutron and a neutral π-meson or into a proton and a negative π-meson, liberating something less than 40 MEV of energy. But it may also exist for a very short time as a constituent particle in a nucleus, simulating an ordinary neutron in its structure. A nucleus so constituted is referred to as a ' hypernucleus ': thus we have the hyperneutrons and the hypernuclei of the title of this section. But physicists are only now beginning the systematic study of these entities, and their further history is for the future.

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Understanding Surface Tension

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Summary

This article discusses the presentation of the facts of capillarity at the level of the upper forms of schools or the first year of a university course. The usual formulae are derived consistently from energy considerations, and care is taken to justify such geometrical approximations as are unavoidable. A plea is made that the subject be taught from the beginning in terms of surface energy, so keeping in sight of physical reality.

1. Introduction

The phenomena which are commonly studied under the heading of ‘ surface tension ’ are properly described as capillary phenomena, or (briefly) as capillarity. Surface tension is a physical quantity, having the dimensions of force per unit length or energy per unit area, which has been introduced on an analogical basis for convenience in handling certain capillary phenomena in an elementary manner. This is not to say that the quantity so designated has no real physical significance. On the contrary, its value is quite definite. It is a property of the particular interface under consideration, and a function of temperature. It is, however, more accurately described as the free (or mechanical) surface energy of the interface per unit area: and as such can be related, qualitatively at least, to the molecular properties of the substances existing on either side of the interface.

2. Facts and principles

The subject of capillarity is normally introduced by the demonstration of a number of phenomena which for convenience may be classified under four headings:

(1) Capillary elevation (or depression). The rise of a liquid in a hair-like tube suggests the name given to the whole group of phenomena (*capillus*—hair, from *caput*—head).

(2) Shapes of liquid drops and bubbles. Attention is drawn to the transition from an approximately spherical to a flattened profile, as the size of the drop or bubble increases. (The soap bubble, being nearly weightless, is a special case.)

(3) Adhesion of solids when wetted (e.g. the hairs of a brush, or straws floating on water).

(4) Spreading of one liquid upon another (e.g. a drop of alcohol on a wet glass plate).

In each case the point is made that the liquid appears to be acted upon by some other forces in addition to gravity; and that when the linear dimensions are small these forces become relatively important. The analogy is then introduced that a liquid behaves as though its surface were a membrane under tension.

The analogy is justified to the extent that it facilitates certain elementary calculations; but we have to face a serious problem if we adopt it as a physical explanation of capillary phenomena. What reality corresponds in fact to the membrane under tension? Do we really suppose that the 'tension' in a liquid surface has the same kind of reality as the tension in an inflated rubber balloon, or in the domestic clothes line? And how are we to account for such a 'tension' considered as a physical reality, in terms of the forces of attraction between molecules? If such a 'tension' is said to arise as the result of intermolecular forces parallel to the surface, how are we to define a boundary layer (distinct from the bulk liquid) in which such a treatment of molecular forces is permissible? And how are we to account for the lower 'tension' when there is a liquid on either side of an interface? The time average of the forces acting on a molecule at any interface necessarily acts at right angles to the interface; and any attempt to explain surface tension directly in terms of such forces is futile. In view of these considerations, it is not desirable that the subject should be taught in terms of a crude mechanical analogy. Capillarity, like nineteenth-century electro-magnetism with its rubber-like tubes of force and rugger-scrum mechanics, has suffered for too long in this way.

How then are we to get past crude mechanical analogies? In exactly the same way as in electromagnetism, by treating the problem primarily in terms of energy. The same group of facts, enumerated above, can be accounted for directly by saying that the total mechanical energy of the system (gravitational plus interfacial) tends to a minimum. In general where a liquid is in contact with a solid there are three phases and three interfaces (solid-liquid, liquid-gaseous, and solid-gaseous). To each interface must be assigned a definite mechanical surface energy per unit area. The concept is, of course, most simple to present in the case of the liquid-gaseous interface; where it is readily seen that mechanical work is done against intermolecular forces when new surface area is created isothermally. The concept, once grasped, is then extended to the solid-liquid and solid-gaseous interfaces. The criterion of minimum mechanical energy is self-evident; and the principle of virtual work (always applicable to a system in mechanical equilibrium) leads readily to the solution of particular capillary problems.

First of all, the general relation between the three interfacial surface energies is obtained without difficulty by the application of this principle. Thus, denoting the free surface energies per unit area by γ_{SL}, γ_{LG} and γ_{SG} respectively we have

$$\gamma_{SG}-\gamma_{SL}=\gamma_{LG}\,.\cos\theta. \qquad (1)$$

The angle of contact θ is defined here as the angle, measured in the liquid phase, between planes tangential to the solid-liquid and liquid-gaseous interfaces respectively at the line of contact. Equation (1) is of general interest, as representing the equilibrium condition of a liquid in contact with a solid; and should always be derived in terms of energy, never in terms of 'tensions'.

The experimental determination of the angle of contact of a liquid against a particular solid normally involves the tilting of a plate until the liquid surface is truly horizontal (as tested optically) right up to the solid surface. The angle is sensitive, however, to the condition of the surface, as would be expected from equation (1) and commonly shows hysteresis effects. Advancing angles of contact are generally larger than receding angles; and it is doubtful whether the concept of an equilibrium angle of contact has strict physical meaning unless the conditions under which it is observed are specified completely. If surface conditions (adsorbed films etc.) and the velocity of advance, or recession, are carefully controlled however, reproducible results are obtainable; but not otherwise.

3. Surface energy and surface tension

Although as a physical account of capillary phenomena the energy approach is infinitely preferable to the tension approach, it is undeniable that there is a formal equivalence between the two, so far as the mathematical development of the theory is concerned. This may be demonstrated by the usual ideal experiment with a film on a wire frame, one of whose arms is movable.

The movable arm is supposed to be entirely without friction. Hence a force must be applied to it in order to prevent the contraction of the surfaces to a state of lower mechanical energy. Let P be the force which is just sufficient to do this. Now let the movable arm be displaced slowly and isothermally through a distance δx so that fresh surface is created. The mechanical surface energy is increased by $2\gamma l \delta x$ where l is the length of the movable arm and γ is by definition the free surface energy per unit area. The work done against the external force is $-P\delta x$. Thus by the principle of virtual work, we have $P = 2\gamma l$. We may interpret this relation by saying that a liquid surface exerts a mechanical force on unit length of any solid body with which it is in contact, numerically equal to the free surface energy per unit area of the surface.

This simple result demonstrates the formal equivalence of the concepts of surface tension and free surface energy, and may be held to justify some of the methods of calculation which are common in elementary treatments of the subject. Such methods, however, generally obscure the physical principles involved; and for this reason are not desirable.

The same ideal experiment may be used to derive an expression for the total energy of a surface. Alternatively, the result may be derived from general thermodynamic relations. By either treatment, we find for the total energy E per unit area

$$E = \gamma - T\frac{\delta\gamma}{\delta T} \tag{2}$$

where T denotes the absolute temperature. The introduction of this result underlines the need to specify that the change in the area of a surface must be made isothermally in the previous calculation.

4. The pressure relation

Although the formal equivalence of the concepts of surface tension and free surface energy is admitted, it is obviously better to derive the pressure relation from energy considerations. To say that a small quantity of liquid behaves *as though* enclosed by a membrane under tension is a fiction; or, at best, an analogy. To say that a pressure difference exists across a curved liquid surface states a physical fact; which can be demonstrated directly with a bubble and a manometer. In developing the subject we proceed therefore to establish the relation between this pressure difference p, and the free surface energy γ of the interface, and the curvature. Whether we restrict ourselves to the simplest case of a spherical surface of radius of curvature r, or treat the general case of a surface having principal radii r_1, and r_2 in two planes at right angles, the relation should be derived without any reference to forces of surface tension.

Consider a rectangular element of the liquid surface, of dimensions x, y sufficiently small to be treated as plane to a first approximation. Let the element suffer an outward displacement δz, under isothermal conditions, so that the area A increases by δA. The mechanical work done against pressure is $-pA\delta z$ and the increase in the free surface energy of the interface is $\gamma\delta A$. Thus,

$$-pA\delta z+\gamma\delta A=0 \quad \text{or} \quad p=\frac{\gamma}{A}\cdot\frac{\partial A}{\partial z}.$$

This relation is quite general; and states the physical principles involved. The rest of the calculation is pure geometry. If the surface is a sphere,

$$\frac{\partial A}{\partial z}=\frac{\partial A}{\partial r}=8\pi r,$$

and hence $p = 2\gamma/r$. For the general surface, let the sides of the rectangle coincide with the directions of principal curvature. Then

$$\frac{\partial A}{\partial z}=y\frac{\partial x}{\partial z}+x\frac{\partial y}{\partial z}=y\frac{x}{r_1}+x\frac{y}{r_2}.$$

Thus

$$p=\gamma\left(\frac{1}{r_1}+\frac{1}{r_2}\right). \tag{3}$$

This is the most convenient form of the general relation between pressure difference and curvature; and throughout its derivation γ has been treated consistently as mechanical energy per unit area of surface, in accordance with physical reality.

5. CAPILLARY ELEVATION

The most elementary treatment of capillary rise equates the forces of surface tension around a circumference to the weight of a column of liquid. This is as misleading as it is unnecessary; since physical principles are obscured. In fig. 1 let the bolder lines represent a vertical tube of circular cross section, OA the axis and AB the liquid meniscus making an angle of contact θ with the wall of the tube. Let the lowest point of the meniscus be A, at height h above the free surface of liquid in a vessel of large area. At A the two principal radii of curvature are equal for reasons of symmetry, i.e. $r_1 = r_2 = r$. Hence, if ρ is the density of the liquid,

$$h\rho g = 2\gamma/r.$$

This relation is absolutely general, and gives the elevation of the liquid in terms of the radius at the lowest point of the meniscus, whatever the actual form of the profile curve.

If we now assume the radius of the tube to be small, the difference in vertical level between A and B is small compared with h. Hence the pressure difference across the meniscus (though it increases slightly as we move from A to B) may be taken as sensibly constant. The liquid surface in the tube is therefore such

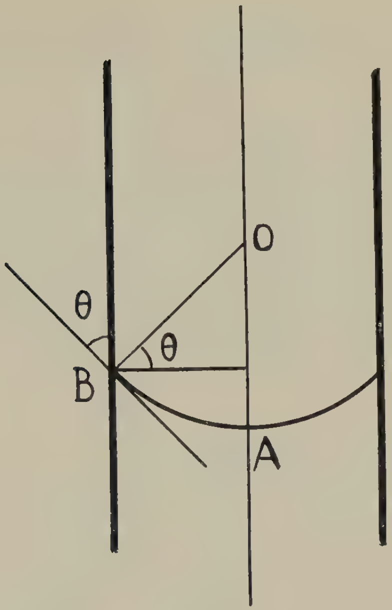

Fig. 1. Meniscus in equilibrium in capillary tube.

that the sum of its principal curvatures is sensibly constant. This suggests (though in strict logic it does not demonstrate) that *each* principal radius of curvature is sensibly constant, and the surface effectively spherical.

Thus, $r = \mathrm{OA} = \mathrm{OB} = R/\cos\theta$, where R is the radius of the tube. This gives the usual relations

$$h = \frac{2\gamma\cos\theta}{R\rho g} \quad \text{or} \quad h = \frac{2\gamma}{R\rho g} \quad \text{for} \quad \theta = 0. \tag{4}$$

The capillary elevation of a liquid between parallel vertical plates of small separation may be treated by the same approximate method as above. Let the bold lines of fig. 1 now represent the two plates. At A, we put $r_1 = r$ and $r_2 = \infty$ giving $h = \gamma/r\rho g$. Again, this result is quite general. If the separation d of the plates is small compared with h, the pressure difference across the liquid surface is sensibly constant as we move from A to B. The profile curve AB is therefore of sensibly constant radius, i.e. it is an arc of a circle. Hence,

$$r = \mathrm{OA} = \mathrm{OB} = d/2\cos\theta$$

giving

$$h = 2\gamma\cos\theta/d\rho g \quad \text{or} \quad h = \frac{2\gamma}{d\rho g} \quad \text{for} \quad \theta = 0.$$

A pair of glass plates separated at one side and in contact at the other can therefore be shown to exhibit the usual rectangular hyperbola as the liquid rises between the plates; and their adhesion is correctly understood as a pressure phenomenon.

Where the radius of the tube, or the separation of the plates, is not negligible in comparison with the capillary elevation, the cruder methods usually attempt to derive a correction term by including the weight of the meniscus in the total weight supported by the surface tension. Nothing that is gained seems to justify a method which loses sight of physical principles. If an accurate treatment is desired there is no satisfactory alternative to the work of Bashforth and Adams.

6. Sessile drop or bubble

In general the height of a drop resting on a plane surface, or of a bubble confined below a plane surface, increases as the diameter increases up to a limiting height, when the free surface is sensibly plane. In the limiting case the height may be expressed in terms of the surface tension by the usual crude method of equating forces; but a far clearer insight is obtained by deriving the equation of the profile curve; and this is by no means difficult.

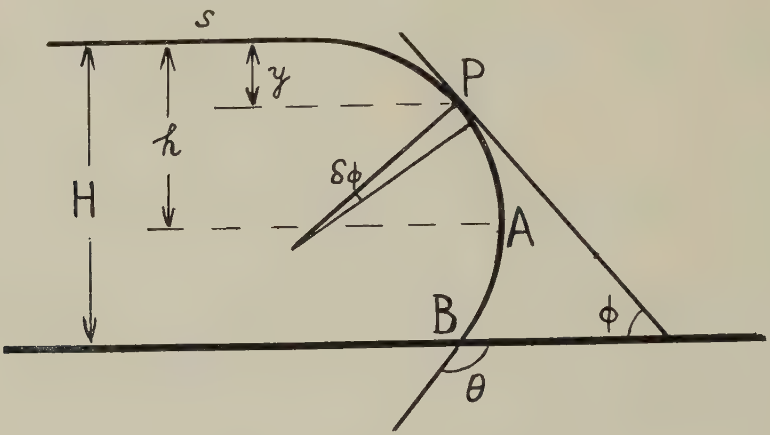

Fig. 2. The sessile drop.

Let fig. 2 represent a section of a drop of large diameter, whose upper surface is treated as plane. Denote a point P on the profile curve by co-ordinates (y, ϕ). Let s denote the distance of P along the profile curve from any origin on the plane surface. Neglecting the curvature in the plane perpendicular to the diagram, the pressure difference across the curved surface is seen to increase with depth in accordance with the relation,

$$p = y\rho g = \gamma\left(\frac{1}{r_1}+\frac{1}{r_2}\right) = \gamma\frac{\partial\phi}{\partial s} = \gamma\frac{\partial\phi}{\partial y}\cdot\frac{\partial y}{\partial s} = \gamma\frac{\partial\phi}{\partial y}\cdot\sin\phi\,.$$

Integration gives

$$\tfrac{1}{2}\rho g y^2 = \gamma(1-\cos\phi).$$

At A, the point of maximum bulge, $\phi = \pi/2$ and $y = h$. Thus,

$$\tfrac{1}{2}\rho g h^2 = \gamma. \tag{5}$$

If H is the maximum height of the drop, i.e. the depth of point B below the upper surface, at which ϕ becomes equal to the angle of contact θ,

$$\tfrac{1}{2}\rho g H^2 = \gamma(1-\cos\theta). \tag{6}$$

Precisely the same treatment may be applied to determine the height of a large bubble confined below a plane surface.

7. THE MEASUREMENT OF SURFACE TENSION

Almost any example of capillarity can be made to yield a formula connecting the surface tension, or free surface energy, with observable dimensions or weight. It seems to be almost inevitable that each one of these formulae should be adapted by somebody to the measurement of surface tension. Some are indeed suitable for the purpose; others are based on such wildly unjustified assumptions that they should be used only with great circumspection. Some of these methods involve an independent knowledge of the angle of contact; or else the assumption that the angle is zero if the liquid wets the solid in contact with it. It will be convenient therefore to look briefly at the better known methods, noting in each case their advantages and disadvantages for reliable experimental work.

7.1. *Capillary rise*

The simplest method capable of giving reasonably reliable results. Clearly a tube of fine bore is necessary if the simple formula (4) is to be used. A fine tube, however, is not easy to clean satisfactorily. Moreover, its diameter is not easily measured, nor its circular cross section tested, at the point at which the meniscus stands. Nevertheless an accuracy of a few per cent is easily attainable with a liquid which wets the tube.

For a higher degree of accuracy Sugden's differential adaptation of the method of capillary rise is available, though tedious. Even so the method suffers from the disadvantage that the liquid surface is not freshly formed, and is therefore liable to contamination.

7.2. *Sessile drop*

Suitable for mercury and liquid metals generally. Equation (5) can be used to determine γ. The measurement of h is not easy; since the surface is highly reflecting, and the point of maximum bulge is not sharply defined in the vertical direction. The angle of contact θ can be determined by eliminating γ from equations (5) and (6).

Other disadvantages of the method are the risk of contamination of the exposed surface, and the need for an empirical correction for drops of finite diameter.

7.3. *Jaegar's method*

The simplest theory of the method is based on the assumption that the pressure in a bubble formed on the end of a fine tube dipping in a liquid reaches its maximum when the radius of curvature is equal to the radius R of the tube, just before the bubble breaks away. The pressure difference may be determined from a manometer reading and the depth at which the bubble is formed. Then $p = 2\gamma/R$. Needless to say, this a very rough approximation.

The method, however, has two distinct advantages which make it worthy of refinement: (*a*) the bubble surface is always freshly formed; (*b*) the radius of the tube is easily determined at the open end. Modified by Sugden as a differential method on the basis of Bashforth and Adams' tables, the theoretical objection mentioned above is overcome, and the method is of great value. Even without these refinements the method can be used to study the temperature variation of γ.

7.4. *Drop weight method*

This is somewhat analogous to Jaegar's method, involving the detachment of a small drop from the end of a tube instead of a small bubble. The simple theory, however, is crude in the extreme, and assumes that the effective radius of the drop at break-away is equal to the radius of the tube. Forces are then equated to give the weight of the drop as $mg = 2\pi R\gamma$. If this result were reliable (which it is not) γ could be found by collecting and weighing a number of drops. However, by representing the ratio of the actual to the 'ideal' weight of a drop as a function of a non-dimensional parameter, Harkins and Brown have adapted this method successfully for comparative purposes.

The chief disadvantage from the experimental point of view is evaporation from the drop ; which must therefore be allowed to form in an enclosed space saturated with its own vapour.

7.5. *Maximum pull on a plate*

Ostensibly this is a direct ' weighing ' of the force of surface tension acting along the two sides of a thin glass slide immersed in the liquid. It is desirable, however, to establish this result rigorously.

Let a plate of mass m and thickness t, be supported by a suitable spring system so that its lower edge is at a depth x below the line of contact of a liquid of density ρ. If the plate suffers a small displacement δx downwards, slowly and therefore isothermally, the energy change per unit length is

$$-(m-\rho tx)g\delta x+2(\gamma_{SL}-\gamma_{SG})\delta x.$$

If the liquid wets the plate, i.e. $\theta = 0$ then equation (1) gives $\gamma_{SG}-\gamma_{SL} = \gamma_{LG}$. The mechanical work done is $F\delta x$ where F is the force exerted by the spring system. Thus, by the principle of virtual work, we obtain

$$F\delta x-(m-\rho tx)g\delta x-2\gamma_{LG}\,.\,\delta x = 0$$

or

$$F = (m-\rho tx)g+2\gamma_{LG}.$$

This shows that capillarity causes a pull on unit length of the plate numerically equal to twice the surface tension of the liquid, as is commonly assumed. As the plate is slowly withdrawn from the liquid the buoyancy term changes sign and the force F increases to a maximum as the plate breaks free. If the plate is 1 mm thick, and break-away occurs with the lower edge 3 mm above the normal surface level, the correction for buoyancy (with water) amounts to 0.03 g, or roughly 30 dynes per centimetre of plate. The surface tension ' force ' is roughly 140 dynes per centimetre of plate. Clearly an exceedingly thin plate is necessary before the correction may be ignored.

7.6. *Ferguson and Kennedy's method*

Suitable for liquids available in very small quantities. This is an adaptation of the method of capillary elevation in a tube, and involves subjecting a liquid index to a measured pressure such that its surface at the free end of the tube is plane (tested optically). The method is subject to the same criticisms as the method of capillary elevation.

7.7. *Method of ripples*

Suitable for mercury. A stationary wave system is set up on the liquid surface by a dipper attached to an electrically driven tuning fork. Frequency and wave-length measurements (n and λ) lead to a determination of γ, since the wave velocity on the surface is given by

$$n^2\lambda^2 = v^2 = \frac{\lambda g}{2\pi} + \frac{2\pi\gamma}{\lambda\rho}$$

The gravity-dependent term is kept relatively small by making λ small, i.e. n large.

8. CONCLUSION

This short article has confined itself intentionally to the teaching of the basic physical principles of capillarity. Numerous applications, and modern developments in the study of surfaces, will doubtless be of interest to the teacher. For these matters reference may be made to the appropriate literature. In particular R. S. Burdon, *Surface Tension and the Spreading of Liquids* (Cambridge Monographs), and N. K. Adam, *The Physics and Chemistry of Surfaces* (Oxford), will be found valuable. The basis of any understanding of capillary and surface phenomena in general, however, is the concept of the free energy of an interface. The purpose of this article is to urge that this basic concept should be adopted from the beginning in the teaching of the subject. To do so does not render the mathematical treatment too difficult for the upper forms of a grammar school: and the teacher who adopts this method will be able to avoid the general mystification which is all too commonly associated with the subject.

Diamond. Part 2

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SUMMARY

A brief review is given of some recent solid-state physics studies made on diamond. These include both the ultra-violet and the infra-red absorption spectra of diamond; the classification of diamonds; a survey of the semi-conducting properties exhibited by some diamonds; diamond luminescence; and some of the effects produced when diamonds are subjected to irradiation by high energy particles and by neutrons. The recently developed diamond radiation counter and the advantages of such a counting system are discussed.

1. INTRODUCTION

An earlier article (Part 1) reviewed some of the sources of diamond, its hardness, its fabrication, and some of its more important uses as a technological material. The review was largely historical. Within quite recent years, indeed the past 10 years largely, the output of research on the physical properties of diamond has grown at a very rapid rate, so much so that industrial diamond abstracts justify a journal to themselves. Current researches on diamond can be broadly classified into those which can be grouped under the heading of solid-state physics and those which are essentially concerned with diamond as a technological hard material. This brief survey deals mainly with the more important of the solid-state physics findings concerning diamond.

There has been considerable recent publication about diamond in the fields of (*a*) absorption spectra, (*b*) semi-conduction, (*c*) luminescence, (*d*) the effects produced by irradiation with high energy particles, and (*e*) the use of diamond as a counter for ionizing particles. The recent technical studies of the industrial applications of diamond include (i) studies on the variation of abrasion resistance in different directions, (ii) investigations into birefringence strain effects (photo-elasticity), (iii) an intensive study of the mechanism of the cutting action and the ultimate wear of the glazier's glass-cutting diamond, (iv) the development of electrical drilling methods for producing diamond dies and jets, a matter of considerable importance in the wire-making industries, and (v) recent improvements in the manufacture and use of diamond-bonded wheels which are so widely used for cutting and slicing, stones, crystals, ceramics, etc. None of these important technological researches will be considered, as this report is restricted purely to solid-state studies.

At the outset, it is most important to recognize that the properties of real diamonds differ widely from those predictable from the crystallographic structure in simple theoretical terms, for there are great variations in many of the measured properties of the crystal. It is almost the case that one does not study the material *diamond*, but one makes observations on different *diamonds*, no two of which are alike. Most properties turn out to exhibit a statistical fluctuation which is most disconcerting if not recognized. It is as if we are never confronted with pure diamond, but always meet diamond with a variable amount of defect, either defect in lattice structure or defect due to impurity inclusion. So tightly bound is the pure diamond lattice, that any defect in the lattice has a profound effect on the physical properties. We shall expect to find then some physical characteristics which are essentially attributable to diamond as such, but often seriously modified because of the inevitable lattice defects.

2. ABSORPTION SPECTRA

There are reasons for believing, on simple theoretical grounds⁽¹⁾ that a perfect diamond crystal, without impurities or dislocations, should be a good insulator, exhibit no absorption of light in the visible region, should not be at all photo-conductive, and should be quite free from optical birefringence. Real diamonds often deviate in many respects from these predictions. The fact that enormous numbers of diamonds are coloured, with hues ranging through the whole rainbow, starting with 'water-white' and ending with a dirty black, shows that there can be a completely variable range of optical absorption in the visible region. However even the most transparent of diamonds, the 'glassies' of the diamond trade, show both intense ultra-violet and infra-red absorption. At least some such absorption is characteristic of all diamond, although the absorption limits can be arranged into discrete groupings. Since 1933 it has been customary to classify diamonds into two types according to their ultra-violet absorption characteristics. The majority of diamonds, no matter how clear in the visible, only transmit ultra-violet light down to a wavelength of about 3000 Å. These are called Type I diamonds. A substantial minority transmit appreciably lower, i.e. down to some 2200 Å and these are called Type II diamonds. The classification is only a rough one, for there are intermediate types; sub-divisions of Type II; and even mixed crystals, part of which have Type I character and part Type II character. Yet this broad classification seems to have a very real physical meaning in terms of concentration of internal defects and dislocations.

All diamonds absorb in the infra-red region between the wavelengths of some 4 and 5μ. In addition to this, the Type I exhibit particularly strong absorption in the wavelength region 8 to 10μ(2). Indeed the infra-red method of classification is the clearer, but it is relatively an easy matter with a small quartz spectrograph to detect the differential ultra-violet absorption and for this reason most workers prefer this simpler ultra-violet method of identification. There is a good deal of independent evidence that the appreciably rarer Type II diamonds approach more nearly to the properties expected for pure diamond. To begin with, most Type II diamonds are small (although a few large ones have been found) and one always expects the smaller crystals to be less subject to defects. Then again, the Type II diamonds have a far more perfect cleavage than do the Type I diamonds. They also have a higher thermal conductivity. The matter is by no means yet clarified, but it is reasonable to state that many Type II diamonds have less defects than most Type I diamonds. Here as always with diamonds, it is fatal to generalize.

Recent work establishes clearly that the extinction coefficient of the 8–10μ absorption band of the Type I crystals is closely related to the position of the ultra-violet cut-off. For there is a real statistical fluctuation in the experimental values for both features, yet at the same time there is a true correlation, as indicated. There are some reasons for believing that both the 8–10μ band and the position of the cut-off appear because there are foreign impurity atoms in the crystal lattice. True they are a characteristic of diamond, but they will not appear in a purer diamond. They are induced as a result of the lattice strains imposed by the accommodation of foreign atoms into the otherwise very tight lattice. It has been suggested, too, that the 4–5μ band, which is common to *all* types of diamond, arises because of the existence of graphite-like carbon groups distributed in the diamond lattice(1). This is by no means an outrageous suggestion. For in a sense diamond at room temperature is metastable. Thermodynamically, at the normal pressures encountered in the laboratory, graphite is the more stable. It is by no means unreasonable to postulate a distribution of local graphitic groupings of carbon atoms, but the matter is still under discussion.

One occasionally finds *blue* diamonds which are of Type II grouping and these particular crystals seem to exhibit special absorption characteristics. For their infra-red absorption spectra are distinguished by the appearance of distinctive sharp lines superposed upon the more normal continuous background (being of Type II this absorption is of course only in the nearer infra-red region). When different crystals are compared the strengths of the lines are found to vary from crystal to crystal(3) but their positions remain unchanged. These blue Type II diamonds, with their special spectra, have one other particularly interesting physical property. They are *semi-conductors*. Resistivity varies from specimen to specimen over a wide range. It seems clear now that the Type II crystals must be subdivided into Type II *a* and Type II *b*, of which more will be said later. The II *a* group exhibit an absorption edge sensitive to both temperature treatment and to irradiation by high energy particles.

3. SEMI-CONDUCTING DIAMONDS

The discovery of the semi-conducting diamonds has aroused a good deal of interest in several connections. Semi-conductivity in diamond was only discovered as recently as 1952(4) and when one considers the long history of research on diamond the lateness of this discovery is clear evidence that we are dealing with what is quite a rare phenomenon for diamond. A small number of semi-conducting diamonds has now been studied. As with other semi-conductors, the electrical resistance drops when the temperature is raised. It has been established that the electrical conduction is of the *p*-type, taking place in the valence band of the diamond. As a general rule diamond semi-conductors are coloured, but one colourless semi-conducting diamond has also been reported(5).

The rather unexpected fact that a diamond can conduct electricity at all can be explained by supposing that there are foreign atoms within the lattice(1), and this seems to be the only plausible explanation for the semi-conductivity. It is strongly supported by the very close correlation between semi-conductivity and phosphorescence. It has long been known that some diamonds phosphoresce when illuminated and that such phosphorescence generally demands impurity centres. In diamond, it has been established that the good conductors are good phosphors and this correlation points very strongly to impurities in the lattice as the common cause for both effects. Again it has also been established that the semi-conducting diamonds are photo-conducting, which can be regarded as further evidence.

Since semi-conductors belong exclusively to Type II they are normally expected to be transparent beyond the infra-red wavelength of 6μ. This is in fact true at room temperature, but absorption begins to appear beyond this wavelength when the temperature is raised. This is a characteristic *only* of the semi-conducting members of the Type II classification, for the non-conducting Type II crystals (by far the commoner) do not show any absorption beyond 6μ when they are heated. We have here then a distinct sub-class of Type II crystals in the semi-conductors. The absorption exhibited by the semi-conductor can only have its origin in the presence of free carriers, presumably the impurity atoms.

4. THE LUMINESCENCE OF DIAMOND

It has long been known that most diamonds are luminescent. In all *but* the recently found semi-conductors there are generally two common emission luminescent centres. When a diamond is suitably irradiated with light it usually emits bands in the two regions near to 5030 and 4150 Å. As is usual with luminescence, the lower the temperature at which a luminescing diamond is maintained, the sharper are the emission bands and the higher the spectroscopic resolution attainable. Both sets of luminescence bands are found to consist of relatively sharp discrete ' lines ' on a broad continuous background. The actual structure depends in every case upon the temperature of the diamond so there are good grounds for believing that a thermal vibrational structure is responsible for the luminescent centres.

Of special interest is the observation that the 5030 Å luminescent centre can be affected by irradiation of a diamond with high energy particles, followed by heat treatment. Now, as will be discussed later, irradiation of a diamond can cause carbon atoms to be knocked out from their places in the lattice, which both leaves vacant sites and also thrusts interstitial atoms into neighbouring parts of the lattice (fig. 1). This then would seem to indicate that the normal λ5030 Å centre arises through such defects. This view is supported by recent studies on luminescence made with polarized light.

But the 4150 Å centre is *not* influenced at all by such irradiation by high-energy particles, so we conclude that the 4150 Å centre is certainly not associated

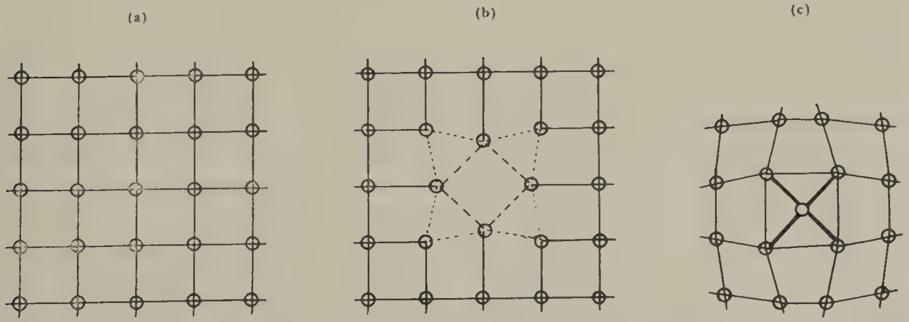

Fig. 1. Schematic effect of neutron bombardment of diamond lattice. (*a*) Normal lattice; (*b*) vacant site; (*c*) interstitial carbon atom. In (*b*) there is " pin-cushion " distortion. In (*c*) there is " barrel " distortion.

with vacant sites or interstitial atoms. This seems a possible field for future investigation.

5. FURTHER EFFECTS OF IRRADIATION

One naturally expects different results from different irradiating particles, and the particular results of irradiation by neutrons, energetic electrons and α-particles have all been individually studied. It will be recalled that graphite is an important moderator in atomic nuclear reactors. Its function there is to slow down neutrons by elastic collisions (the simple mechanics of elastic impact holds, and shows that the energy lost by a neutron per collision is greatest for light nuclei) and it is selected for this purpose both for its low atomic weight and also because its cross-section for (i.e. tendency to) neutron capture is small. From the viewpoint of nuclear collision processes there is no effective difference between a carbon atom in a *graphite* crystal lattice and a carbon atom in a *diamond* crystal lattice. Diamond will therefore behave the same way as does graphite when subjected to neutron irradiation. If then a diamond is placed in an atomic reactor and subjected to intense neutron bombardment one can anticipate what will happen. The reactor neutrons will treat the diamond in the same way as they treat graphite, and will suffer elastic collisions and not capture. Elastic collision between a neutron and a carbon atom causes two types of lattice distortion in the diamond. At each collision a carbon atom is ejected from its position in the lattice. This produces *a vacant site*, a hole, which leads to local dislocation in the array of atoms forming the crystal. The ejected carbon atom comes to rest in the crystal at some distance from its original site and is forced into the lattice as an *interstitial* carbon atom. This again produces a local distortion in the lattice array, but in a sense of opposite sign to the vacant site. The interstitial atom is only accommodated by a distortion of the normal bonds holding the crystal together, so again a lattice dislocation results.

Because of the relative infrequency of nuclear capture, due to the high probability of mere elastic collision, a diamond after exposure in an atomic reactor is hardly radioactive at all; indeed any residual activity may well be largely due to irradiation of foreign atoms in the original diamond. This fact makes it easy and safe to study diamonds which have been subject even to intense neutron bombardment.

Neutron bombardment, with its attendant resultant vacant sites and interstitial carbon atoms, is found to produce a strong green colouration. Increasing the dose leads ultimately to complete blackening of the diamond, presumably because of the amount of ‘ free ’ carbon liberated. There has been some mild excitement amongst diamond dealers about this production of green colouration by neutron irradiation. Green diamond *gem*-stones, if of good transparent quality, come into the trade classification of ‘ fancies ’ and as such fetch an appreciably higher price than a corresponding uncoloured stone. It was argued that it would pay to colour stones by irradiation. Yet it has now been established that a diamond coloured green by neutron irradiation loses its colour if heated, say to 450°c. Such an annealing, in effect, increases the thermal motions of the carbon atoms. This leads to an increase in mobility which enables each of the interstitial carbon atoms to drift to the nearest available vacant site (it need not be its own parent site, which may well be quite distant).

The interstitial atoms immediately fill the available vacancies, and so the whole green colour is destroyed, since its very existence depends on the vacant sites and interstitial atoms. This easy way of establishing that the colour is radiation induced has somewhat shaken the financial optimists. It will be recalled that in connection both with the synthetic ruby trade and with cultured pearls, once it was established that there is a readily observable distinction between the synthetic and the ' natural ' product, the price of the synthetic product collapsed. Dealers and buyers have a horror of a gemstone that may be tainted with the suspicion of being other than ' natural '. The buyers might well be inclined to apply the same arguments to diamonds turned green ' artificially '.

Irradiation by neutrons has an unexpected and interesting effect upon the abrasion resistance (hardness) of diamond(6). In general, it is usually found that irradiation of a metal by reactor neutrons leads to the creation of defects of the kind which can resist the movements of dislocations. Clearly anything which resists the movement of those dislocations always present in metal crystals has in effect produced a *hardening* of the metal. It is therefore not surprising to find that a certain amount of neutron irradiation hardens metal crystals. In the case of diamond, irradiation has been found on the contrary to produce a *reduction* in resistance to wear. In the particular experiments under discussion the wear was measured by determining the quantity of diamond abraded away under standardized abrading conditions. The amount of wear from a crystal was found to increase with the amount of exposure to radiation. For an exposure to a neutron flux (classed as 6×10^{20} nvt fast) the grinding hardness fell to only 25 per cent of the grinding hardness of the original diamond. Linked with this reduction in strength was the interesting observation that after irradiation the density had diminished. The above radiation flux produced no less than a 4 per cent reduction in density. It is not surprising that there was a corresponding reduction in grinding strength.

It is possible, from the known flux and the known distribution of carbon atoms in the lattice, to make some approximate estimate of the expected decrease in cohesive energy in an irradiated diamond, for the number of broken bonds expected can be approximately computed. Such a calculation, when applied for the flux just mentioned above, leads to a prediction of a reduction in strength of some 40 per cent. In view of both the nature of the approximations and the difficulty of measuring grinding hardness, this figure is in very reasonable agreement with the experimentally determined reduction of 75 per cent.

In addition to those radiation studies made with reactor neutrons, a number of reports have appeared on the behaviour of diamonds subjected to bombardment by high energy electrons. The effects produced are complex and have been shown to influence the positions and intensities of the lines of the fluorescence spectrum. The effects found depend in a marked way both upon the energies of the bombarding electrons and upon the temperatures of the crystals being irradiated.

6. DIAMOND RADIATION COUNTERS

The discovery of the crystal counter goes back to 1945(7) when it was reported that a crystal of silver chloride maintained at a low temperature (e.g. temperature of liquid air) could be used as a detector of β-radiations. Since that date several other types of crystal counter have been described and perhaps the most notable of these is the diamond counter. For it has been found that a small number of Type II diamonds can be made to count ionizing particles and ionizing radiations in a most effective manner. Just as is the case with semi-conductivity, so with the counting facility there is very great variability in efficiency among crystals(8). The notable advantage of diamond as a crystal counter is that it functions well even at *room* temperature. The high chemical and thermal stability of diamond makes the diamond counter a detector of special value under unusual corrosive or high temperature conditions. Because of the excellent counting facility of even quite small diamonds it is possible to construct practical counters for use for pin-pointing quite small local sources of radiation(9). Counters of such small dimensions have obvious particular advantages in biological research, especially in connection with the adsorption of radioactive tracer elements in localized parts of the body. Also, a diamond counter can easily be made sterile for internal medical and biological studies. It seems clear then that the diamond counter has quite a future for special researches.

It has been found that α- β- and γ- radiations can all affect a diamond but as prolonged irradiation usually leads to the appearance of a restrictive polarization effect (of which more will be said later) special arrangements must often be adopted to counteract this effect. Good counting diamonds are very rare, indeed the counting facility is apparently even rarer than the semi-conducting character. The impact of a single ionizing particle on a counting diamond leads to local production of ions. If the counting crystal is maintained in an electric field a small current-pulse results. The pulse is so small that special attention must be paid to the amplifier circuit (fig. 2) used to magnify the pulse. In particular the impedance of the diamond holder, and of the cable circuit between diamond and amplifier, must be reduced to a very low value.

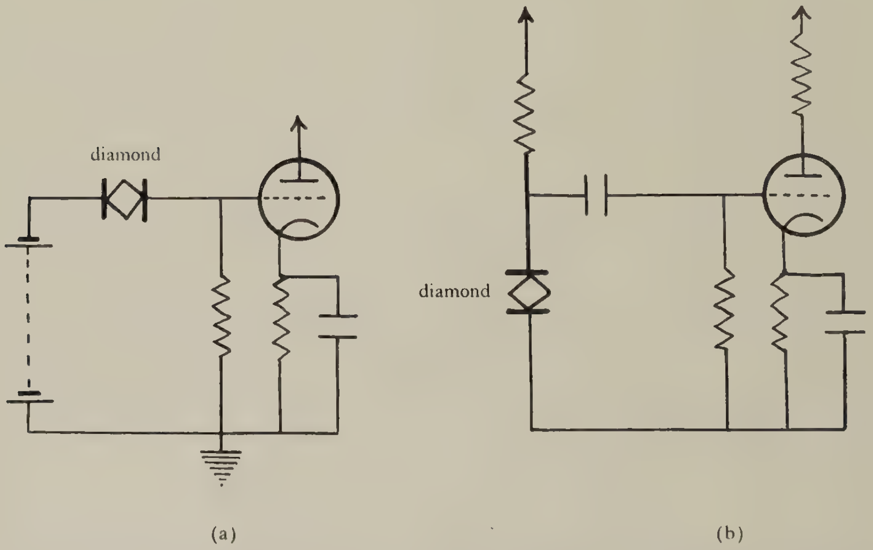

Fig. 2. Detector circuits suitable for a diamond counter: (*a*) with separate high-tension for diamond; (*b*) with diamond high-tension taken from normal power pack supply. To be followed by high-gain amplifier.

In very general terms the counting property of a diamond can be described as follows(9). The bombardment of the crystal by the high energy particle raises electrons into a conduction band. A whole shower of electrons is liberated along the length of the track of the incident particle, but associated with these there are left behind an equal number of positive holes. (This is reminiscent of the mechanism previously described for the neutron irradiation of diamond. There it was whole carbon *atoms* ejected from a lattice leaving vacant sites, but here it is merely *electrons* ejected from the lattice leaving electrically positive holes.) The symmetry of the diamond crystal is such that the mobilities of the freed electrons and of the residual positive holes are closely comparable. If then the crystal be maintained between high potential electrodes (something of the order of 1000 volts is ample) there will be a drift of charges in opposite directions, constituting a current. Practically, the simplest device is to cut a thin slice of the crystal, and then to coat opposite faces with thin metal layers, these two films constituting the electrodes. The system is placed in the grid circuit of an amplifier and so the small current-pulses are amplified and can be registered, for the flow of electrons and holes constitutes a conduction current. Either direct mechanical counting systems, or alternatively, an oscilloscope display, can be used to indicate and register the oncoming ionizing radiations.

All crystal detectors, the diamond counter included, suffer from the defect that even mono-energetic incident ionizing particles produce within the crystal pulses of different amplitude. There is in fact a statistical fluctuation both in the number of primary particles created and in the ultimate current registered. If single particles are being detected, and their specific energies do not matter, then this is no drawback for then the number of pulses and not the pulse height is the significant matter. If on the other hand the intensity or energy intensity is also required, then only the pulse *maxima* should be noted and these maxima are to be taken as characteristic for the source. For clearly the maxima will be a measure of the intensity of the incoming radiation, whilst all values below this are essentially due to the statistical effects.

Current fluctuations arise not only from the statistical fluctuation in the number of the primary particles; they can also be caused because crystal imperfections interfere with the free migration of the liberated ions. Lattice defects, dislocations, impurity centres or even micro-cracks, all interfere with free flow, especially of the electrons. They act as what are called ‘ electron traps ’. The traps play a basic part in the production of the state of polarization, a state which can impede and even stop the counting mechanism. Some trapping there must always be, but a good counting crystal is one in which the trapping machinery is least effective, and ways and means of reducing electron trapping have been studied. Trapping is reduced, to begin with, by using a sufficiently high potential across the crystal. By this means the velocity of the ions is increased. They are therefore only in short time-contact with the trapping regions; in fact they move fast enough to pass through the trapping barriers. There is a limit to this effect, which operates up to a certain voltage, beyond which there is a saturation and after which there is no gain.

The density of a diamond is such that the lower-energy particles have a very short range in the crystal through which they pass to give up the whole of their energy. Even α-particles penetrate a mere 10^{-3} cm, this being the whole of their range in diamond. Arrangements are usually made for the point of entry of the ionizing particles to be close to the cathode, but desirable as this may be, one finds in practice that there is a restriction imposed by the very considerable variability in counting facility. For it is found that there can be quite a big variation from place to place even on a single crystal and clearly one is obliged to select a sensitive region, which may or may not be quite close to the cathode.

The extreme variability in the counting facility deserves especial mention. Many Type II diamonds are counters of sorts, but very few indeed make good counters. The necessary conditions are severe(10). First the crystal must be a good insulator. Clearly then the counting crystals are *not the semi-conducting crystals.* Second there must be very few electron traps. This implies a crystal of inherently fine quality, free from impurities and also from growth dislocations, or at least relatively free from these defects. It has been found that electron traps can often be reduced by a strong thermal annealing treatment, hence the selected crystal must be thermally stable to permit it to withstand such treatment. Finally, each primary particle should produce many secondaries, although it must be admitted that the secondary-producing mechanism has not yet been explained.

There is little guide as yet to selection of the best diamonds for counters. Indeed one is virtually obliged to use the crude sledge-hammer tactic of trying out hundreds, if not thousands, in a counter circuit in the hope of finding some that work! There are one or two guiding clues. A close correlation has been found between counting efficiency and the position of the ultra-violet cut off(11), but this hardly works in reverse; all that can be said is that the more transparent the crystal to the ultra-violet the better the counter. Then again it has been established that good counters are usually free from birefringent strains, whilst non-counters often show strain patterns, but again this does not work in reverse, i.e. a diamond free from strain is not automatically a good counter. A further relation found is that good counters do not fluoresce under ultra-violet irradiation. All that these correlations really mean is that the good counter is free from defects, but to be relatively free from defects, as we see, does not guarantee good counting. All in all, selection tediously by the counting property itself is the remaining practical guide in the end.

The relative rarity of good counting diamonds is made clear from a report(9) of an examination of some 7000 diamonds for counting properties. Of these only 70 were found to be counters, and all indifferent counters at that, excepting four, which were really good and suitable as practical counters. The main difference between the good and bad counting diamonds was principally that of polarizability. The four good diamonds showed good counting rates and maintained those rates for continuous radiation up to as long as 12 hours, without any observable polarization effects setting in. They were quite exceptional, for the majority of the counters, even if good to start with, polarized in a matter of minutes, indeed in some cases in a matter of seconds. In this respect there is some evidence that the smaller the diamond the better is the performance, and this is not unexpected if defects are responsible for the polarization.

One piece of evidence has recently emerged which gives some indication as to what is the special peculiarity which makes a counter. There is evidence that a counting diamond is one that has a marked *laminated* structure. It is a diamond that apparently consists of alternations of very perfect diamond separated by layers, though the nature of the separating layers is not yet clear. It may well be that a layer of perfect single crystal has been laid down, then the next layer fails to key perfectly, i.e. there is some imperfection in lattice fit. This acts over a very thin region and once again the perfect layer is put down. It seems clear from the physical properties of the counter that these imperfect layers between the good thick layers are not to be considered as impurity atoms; they are probably pure diamond, but the keying action between planes has gone wrong. This is as yet a mere conjecture, although it is considered that a succession of laminae only 10 microns thick each would satisfactorily account for counting properties.

It has already been pointed out that some diamonds occur which are a mixture of Type I and Type II. Such mixed types can be counters, but when they are, the counting applicability depends upon the location of the Type II region relative to the surface. If the Type II region is near the surface then the diamond can be used to count α-particles, but if it is deep down then it can only be used for β- or γ-radiations, since all α-particles would be absorbed before reaching the counting-sensitive region.

The accumulation of space-charge in the traps in the case of all but the very best of counters is responsible for the polarization which can so severely reduce an initially high counting sensitivity. Ion-trapping is the basic cause of this polarizability, for traps retain the free carriers of charge. Quite a number of different successful methods have been developed for reducing the polarizability, but it should be mentioned that there are a few high-quality counting diamonds which do not need these elaborations. Some de-trapping can be secured by irradiating the crystal with light, especially infra-red light. Also the use of light flashes alternating with suitable electric fields has yielded reasonable results. Pulsed a.c. fields of special wave form, produced from special circuits have also been found useful.

With the non-polarizing special diamonds, or with those in which polarizability has been effectively reduced, very rapid counting becomes possible. Indeed diamond counters have been made with the remarkably low resolving time of 10^{-8} second. This is better than the Geiger counter.

Compared with the gas-filled Geiger counter, the diamond counter has considerable advantages when very high energy particles are being studied. The density of diamond is such as to stop completely a one million volt electron in a path length of only 1 mm. It is this stopping power which makes a diamond counter more efficient than a Geiger counter for recording the higher energy particles, yet even for very low energy particles the efficiency of the diamond counter is comparable with that of the Geiger counter.

Mention may finally be made of one peculiar characteristic of those Type II diamonds which are classed as not being true insulators. Some, as already pointed out, are semi-conductors. Some(12) have the remarkable property of being *actual conductors* with resistance a mere few ohms per cm and capable of passing heavy currents. The majority of the Type II diamonds have extremely *high* electric resistance, something of the order of 10^{14} ohm/cm. There is some slight indication that counting diamonds have resistances which lie *between* those of the semi-conductors and those of the insulators, but the evidence as yet is not strong.

It seems quite probable that diamond counters will find many special uses in the future. Their extreme lightness and mechanical robustness, for instance, suggest that given the necessary lightweight transistor circuitry, they would be ideal instruments for space rockets.

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Primary Cells

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Summary

The systems of the Leclanché, magnesium and R.M. (mercury) cells are described in detail, together with the lesser-known air depolarized and reserve type cells. Nuclear cells are still in the development stage and will be used for specialized applications only. Fuel cells are of great interest to the electrical engineer and a great deal of work has been done on this type of cell resulting in the recent production of a practical operating unit.

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1. Introduction

A cell may be defined as a device for producing electrical energy directly from a chemical reaction. A primary cell is used for the production of electrical energy only and the final condition of the cell may not be restored to the original by 'recharging' or replacing the electrical energy from an external source. It is true to say, however, that most primary cells may be 'recharged' through some limited part of the discharge process for a limited number of cycles.

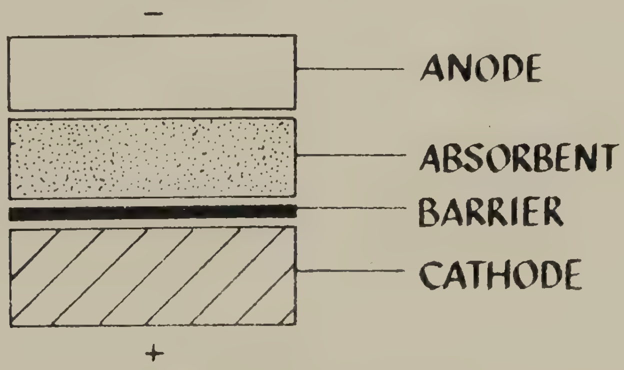

Fig. 1. Basic components of a primary cell.

The basic components of a primary cell are shown in fig. 1. These components are arranged in the configuration shown with the anode and depolarizing cathode presenting as much surface area towards one another consistent with good cell design. Anode and cathode are separated by an absorbent barrier or absorbent plus a barrier, containing the cell electrolyte. The electrolyte in some cases is 'gelled' with starch or carboxymethyl cellulose and under these conditions all or part of the absorbent and barrier may be omitted. The barrier is a dialyzing membrane and is generally placed directly onto the depolarizing cathode to prevent the migration of the micronized graphite or fine metal powders that are often incorporated in depolarizing masses as an aid to conductivity. The anode is manufactured from a metal which lies near to the negative end of the electrochemical series (i.e. a base metal), and may be manufactured as metal particles sprayed or plated on to a former, sheet metal, corrugated sheet metal or a sintered pressed powder block. The depolarizing cathode consists of a reducible (i.e. oxidizing) compound plus a conductor and must be electropositive (i.e. noble), with respect to the anode. Commonly used depolarizing agents are the higher oxides of metals. The electrolyte is almost invariably a strong aqueous solution with a high electrical conductivity. The choice of solutes is therefore restricted to strong electrolytes which do not react spontaneously with the other materials of the cell or, at least, can be prevented from doing so.

The theoretical potential difference between the electrodes of any primary cell may be calculated from free energy data; when the cell operates reversibly (i.e. under no-load conditions), the electrical work done is a maximum and is equal to the free energy change of the reaction. The overall cell reaction must be considered and the total free energy change ΔF calculated from free energy of formation tables when

$$\Delta F = -z\,E\,F$$

where ΔF = free energy change for overall cell reaction in joules, z = number of gram-equivalents participating in chemical action, $\boldsymbol{F}$= the Faraday (96 484 coulombs).

The equation becomes,

$$\Delta F \text{ (kcal)} = -zE \times 23{\cdot}06.$$

For a straightforward case, agreement between the no-load voltage, as measured by potentiometer, and the theoretical potential difference is good. An example is a zinc/mercuric oxide alkaline cell of the R.M. (Ruben–Mallory) type, which has the overall cell reaction:

$$\text{Zn} + \text{HgO} + \text{H}_2\text{O} \rightarrow \text{Zn(OH)}_2 + \text{Hg}.$$

With all components considered in their standard states at 25°C, the individual ΔF values are: Zn=zero; HgO=$-13{\cdot}99$ kcal; $H_2O = -56{\cdot}69$ kcal; $Zn(OH)_2 = -132.6$ kcal; Hg=zero. So, for the whole,

$$\begin{aligned}\Delta F &= -132{\cdot}6 + (13{\cdot}99 + 56{\cdot}69) \text{ kcal}\\ &= -61.92 \text{ kcal}\\ &= zE \times 23{\cdot}06.\end{aligned}$$

Two gram-equivalents participate in the cell reaction represented, and therefore

$$E = \frac{61{\cdot}92}{46{\cdot}12} = 1{\cdot}34 \text{ v.}$$

Measured values of no-load voltage on R.M. cells give approximately 1·35 v.

The theoretical capacity, (which means the quantity of electricity, measured in coulombs or else in ampere-hours, that it can circulate) of a primary cell can be estimated using Faraday's Laws. One gram equivalent of active material yields 96 484 coulombs. Zinc is often used as the anodic metal in primary cells and has an equivalent weight of 32·69 g.

$$32{\cdot}69 \text{ g of zinc} = 96\,484 \text{ coulombs (amp sec) of electricity;}$$

$$\begin{aligned}1 \text{ g of zinc} &= \frac{96\,484 \times 10^{-3}}{32{\cdot}69 \times 3600} \text{ milliampere-hours}\\ &= 819 \text{ milliampere-hours.}\end{aligned}$$

Provided that the depolarizer is in stoichiometrical excess, as shown in the overall cell reaction, the theoretical cell capacity can be calculated from the anode metal weight in the cell. Should the anode metal be in excess, the cell capacity can be calculated in a similar manner, but based on the depolarizer weight, or even in some cases on the electrolyte weight and composition, where this is consumed in the cell reaction.

The realizable capacity of a primary cell is easily found experimentally by discharging a cell through a fixed resistance, preferably 25% of the manufacturer's recommended maximum load, and plotting the voltage drop across the resistance against time. The area under the curve with the potential difference in volts plotted against the time in hours, divided by the load resistance in ohms gives the realizable cell capacity in ampere-hours.

Anodic materials are generally electronegative metals and they are characteristically electron donors, dissolving in the electrolyte to form positive ions. The process occurring at the anode is one of oxidation. The anode is conventionally the negative terminal of the cell.

Cathodic depolarizers are generally oxides or chlorides of metals, and they are characteristically electron acceptors, being reduced in the process to the metal, or to lower oxides in the case of metal oxide depolarizers. The depolarizing cathode is conventionally the positive terminal of the cell.

Both electrodes are theoretically at equilibrium with the electrolyte when the cell terminals are disconnected, but in almost all cases, parasitic corrosion, reaction or dissolution may occur at one of the electrodes, thus limiting the ' shelf-life ' of the cell. When the terminals are connected through an external circuit, current flows and is carried *within the cell* by the ions in the electrolyte, the negative ions passing to the anode, and the positive ions to the cathode. In the external circuit, the flow of electrons passes from anode to cathode and the conventional direction of current from positive to negative terminals.

When current is drawn from a cell, the potential difference between the terminals falls to a value known as the ' on-load voltage '. The potential drop between ' no-load voltage ' and ' on-load voltage ' has three major components: the *IR* drop across the resistance of the cell components remote from the active electrode faces, the activation over-voltage at both electrodes and the concentration polarization effect. Activation over-voltage at an electrode is associated with a free energy barrier opposing ion transfer at the active electrode face, with a resultant potential drop across this barrier. The magnitude of activation over-voltage can generally be reduced by lowering the current density on the electrode, or in some cases by increasing the temperature. Depolarization is the only effective way of reducing hydrogen over-voltage at cathodes.

Concentration polarization is associated with the local reduction of ion concentration at an active electrode face. In effect this produces local concentration cells with an e.m.f. operating in opposition to the ion flow. These local concentrations disperse by diffusion and convection when the cell is taken off load. Concentration polarization may be minimized by increasing the bulk concentration of the electrolyte, by increasing the temperature, or by opening up the electrode structure to allow better electrolyte circulation. The contribution of concentration polarization to the on-load voltage drop can be considerable, particularly at low temperatures.

Primary cells invariably have cathodes which are depolarized. The actual evolution of hydrogen at a cathode is usually associated with high activation over-potential, and in neutral or alkaline primary cells molecular hydrogen is not formed. Hydrogen discharge is assisted by the depolarizing material which removes hydrogen atoms, becoming reduced itself in the process. The discharge of hydrogen in neutral or alkaline cells can be regarded as the adsorption of a hydrogen atom from a water molecule by the cathode conductor, followed by the reaction of the adsorbed hydrogen atom with the depolarizer, e.g. for the R.M. cell system at the beginning of discharge,

$$\text{graphite} + H_2O + e \longrightarrow (\text{graphite - - - - H}) + OH^-$$

followed by

$$2(\text{graphite - - - - H}) + HgO \longrightarrow H_2O + Hg + 2(\text{graphite}).$$

2. THE LECLANCHÉ CELL

The Leclanché cell in its original form was first produced in the mid-nineteenth century and consisted of a zinc rod (anode), a solution of ammonium chloride (electrolyte), and a depolarized cathode consisting of a carbon rod surrounded by a packing of powdered manganese dioxide within a porous pot. This cell provided an effective source of electrical energy and was subsequently developed to produce the dry cell that we know today which, though basically unchanged, bears little physical resemblance to the original wet Leclanché cell.

A typical Leclanché cell is shown in fig. 2.

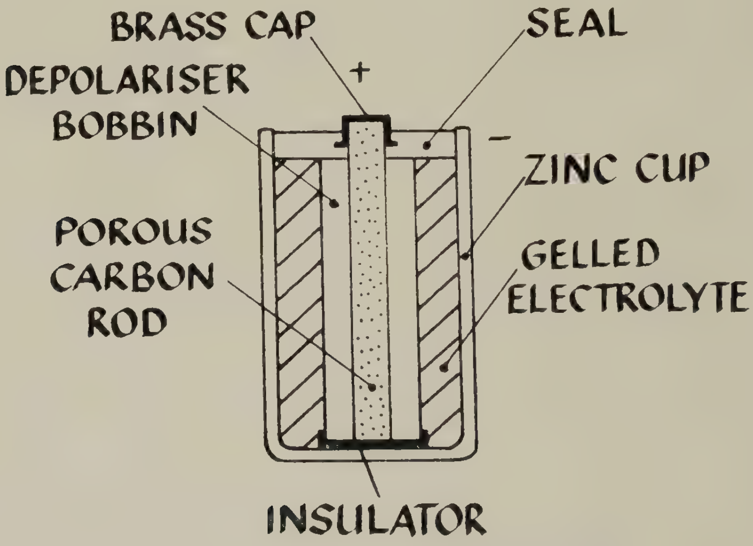

Fig. 2. Diagram of a Leclanché cell, simplified.

The materials for dry Leclanché cells are carefully chosen to produce optimum performance and shelf-life. The seamless zinc container is impact extruded and the zinc used is alloyed with about 1·0% lead and about 0·05% cadmium, both to improve drawing qualities and reduce electrolyte penetration during storage. The iron and copper content of the zinc must each be reduced to less than 0·005% to allow easy extrusion; both these metals also increase the speed of perforation. Tin, although not one of the usual impurities in pure zinc, must be kept below 0·001%, quantities higher than this make the metal unworkable.

The depolarizing mix consists of manganese dioxide (approximately 60%), carbon as graphite and/or acetylene black (up to 20%), ammonium chloride (10 to 20%), water (10 to 20%) and zinc chloride (approximately 1%). This mixture is thoroughly ground and then pressed to form bobbins into which the central rod of porous carbon is inserted. The type of manganese dioxide used is of some importance and affects the discharge performance; electrolytically produced manganese dioxide is best, but the cost of this material is much higher than the natural manganese dioxide (pyrolusite) and the chemically activated ore. Acetylene black is said to give a better liquid retention in the depolarizing bobbin than graphite; various mixtures of graphite and acetylene black are used, depending on the cell characteristics required. Carbon rods are carefully manufactured and must be porous enough to vent gases generated within the cell, but at the same time they must not soak up the cell electrolyte. The make-up of the depolarizing bobbin is such that the cathode reactions occur over its extended surface area which is well wetted with electrolyte. Little action takes place at the central carbon rod which is a gas venting mechanism and current collector.

The electrolyte is a gelled solution of ammonium chloride in water and a typical electrolyte formula might have the following composition:

26% ammonium chloride, 9% zinc chloride and 65% water.

A small quantity of mercuric chloride is added to this solution and the whole is ‘ gelled ’ with a mixture of starch and wheat flour. The zinc chloride assists in the gelation of the electrolyte and the process is completed by heating. Zinc chloride also reduces the corrosion of the zinc can by ammonium chloride during storage and is hygroscopic, thereby assisting water retention in the gelled electrolyte. Mercuric chloride (approximately 0·1% of the can weight) is added to amalgamate the inner surface of the zinc can which inhibits corrosion due to the high hydrogen over-voltage on mercury; the wheat flour present in the gel is yet another corrosion inhibitor. The presence of all three aforementioned substances, used to prevent attack on the zinc can, is essential to obtain an acceptable shelf-life for Leclanché cells. Gelled electrolyte acts as an effective barrier between anode and cathode.

The reactions taking place within the cell are complex and the mechanism of the cathode reaction obscure. Various schemes have been suggested for the cell reactions and a simplified form of one of these follows:

(1) Overall cell reaction:

$$\mathrm{Zn} + 2\mathrm{NH_4Cl} + 2\mathrm{MnO_2} \rightarrow \mathrm{ZnCl_2} + 2\mathrm{NH_3} + \mathrm{Mn_2O_3} + \mathrm{H_2O}.$$

(2) Reactions at the anode:

$$\mathrm{Zn} - 2\mathrm{e} \rightarrow \mathrm{Zn^{++}}.$$

Ammonium chloride dissociates to form ions:

$$2\mathrm{NH_4Cl} \rightarrow 2\mathrm{NH_4^+} + 2\mathrm{Cl^-}$$
$$\mathrm{Zn^{++}} + 2\mathrm{Cl^-} \rightarrow \mathrm{ZnCl_2}.$$

The anodic product is therefore initially zinc chloride.

(3) Possible reactions at the cathode:

$$2MnO_2 + 2NH_4^+ + 2e \rightarrow Mn_2O_3 + 2NH_3 + H_2O$$

and

$$2MnO_2 + H_2O + 2e \rightarrow Mn_2O_3 + 2OH^-$$

The hydroxyl ions liberated at the cathode convert ammonium chloride to ammonia and chloride ions which diffuse to the anode and form an insoluble complex salt (diammine zinc chloride) with the free zinc ions:

$$Zn^{++} + 2NH_4Cl + 2OH^- \rightarrow Zn(NH_3)_2Cl_2 + 2H_2O.$$

These cell reactions have been simplified in order to understand the basic cell reaction. Other compounds formed in the cell during discharge are basic zinc chlorides and a compound of zinc oxide and manganic oxide. The final end products of the cell reaction also depend on the conditions of discharge.

Figure 3 shows a typical discharge curve for a Leclanché cell on continuous drain at room temperature; the no-load voltage of an unused cell, off load, is between 1·50 and 1·65 v.

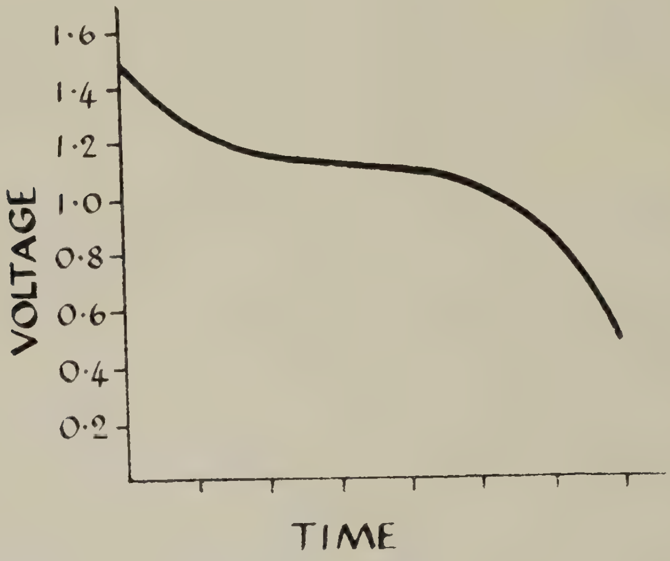

Fig. 3. Continuous discharge curve for a Leclanché cell at room temperature.

Leclanché cells are able to operate intermittently on quite heavy drains with good recovery during rest periods. Owing to local action the full cell capacity calculated on active components is not realized. Capacity is obviously not dependent on zinc weight since the zinc can is also the cell container and must not be weakened excessively during discharge. The discharge life is generally limited by the accumulation of discharge products and local action at the zinc can.

An unused cell deteriorates on storage and the no-load voltage falls as the cell becomes older. The main factor determining the storage life is the extent of attack upon the zinc can by the electrolyte; and indication of how this is minimized has already been given. The zinc can will become coated with diammine zinc chloride and will eventually perforate; this process increases in speed with increasing temperature.

$$Zn + 2NH_4Cl \rightarrow Zn\ (NH_3)_2Cl_2 + H_2.$$

Conversely, storage at low temperatures increases the ‘ shelf-life ’ of Leclanché cells.

The second factor is the drying out of the electrolyte, but generally cell sealing is very efficient and this factor plays little part in storage life nowadays.

Discharge performance of Leclanché cells at low temperatures is unsatisfactory and only low current drains may be used below 0 °C. At about −20 °C the normal Leclanché electrolyte freezes and thereafter the cell becomes useless for normal applications. Low temperature electrolytes in which the ammonium chloride is partially replaced by lithium chloride give an improved performance but even so, capacities obtained from these cells are low when compared with room temperature performance.

The layer type Leclanché cell is constructed as in fig. 4.

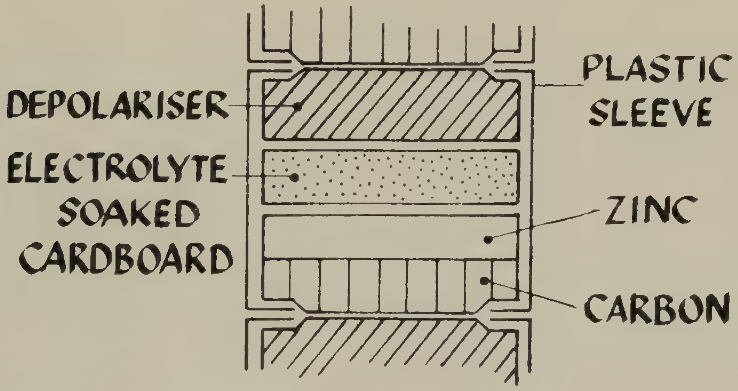

Fig. 4. Layer type Leclanché cell.

Active materials are almost the same as for other types of Leclanché cell and the only novelty lies in the construction of these layer cells, from which compact high voltage batteries can be built. The zinc anode is coated on one face with an impervious layer of carbon, which makes contact with the depolarizer block of the next cell. This carbon coated zinc is known as a ‘ duplex ’ electrode. A cardboard strip holds the electrolyte and also separates the zinc face from a compressed block of depolarizer. A whole unit cell is partially enclosed with a plastic sheath leaving only the carbon face at the negative end of the cell and a depolarizer face exposed at the positive end of the cell. These unit cells are stacked under pressure with depolarizer faces making contact with the carbon faces of adjacent cells, i.e. they are stacked in series. The whole stack is taped to keep each cell in pressure contact with the next and wax coated to seal off the unit. These battery stacks are used for high-voltage, low-current batteries. The commonest battery of this type is a 22·5 V battery and the current drawn from this type is usually not more than 20 mA.

3. THE MAGNESIUM CELL

The magnesium cell has the same physical construction as the Leclanché dry cell, but the cell can is impact-extruded from a magnesium alloy slug containing about 3% aluminium and 1% zinc. The electrolyte is a gelled solution of about 20% magnesium bromide with the addition of a small quantity of a soluble chromate as a corrosion inhibitor. The depolarizer is of the manganese dioxide/carbon type and its method of manufacture very similar to the Leclanché depolarizer, with a central rod of porous carbon passing through the depolarizing bobbin. The cell is vented to allow the escape of hydrogen generated at local cells on the internal magnesium face. A simplified overall cell reaction may be written:

(1) $Mg+2MnO_2+H_2O \longrightarrow Mg(OH)_2+Mn_2O_3$.

(2) Reactions at the anode: $Mg-2e \longrightarrow Mg^{++}$

$$Mg^{++}+2OH^- \longrightarrow Mg(OH)_2.$$

The anodic reaction product is magnesium hydroxide.

(3) Reactions at the cathode:

$$2MnO_2+H_2O+2e \longrightarrow Mn_2O_3+2\,OH^-.$$

The open circuit voltage of the magnesium cell is about 1·8 v but when the cell is put on load, the true voltage level is not attained until the effect of the passivating chromate film on the magnesium can has been overcome. This delay is a characteristic of the magnesium cell and its length depends to some extent on the current drain from the cell in relation to its size, the age of the cell, the conditions of cell storage off load and the length of time that the cell has been off load. This delay may be only of the order of seconds and in many cases this is not a serious drawback for a cell application. Increasing the current density on the magnesium surface and increasing the temperature will reduce the delay period.

The advantages of magnesium over zinc as an anode metal are its low equivalent weight and its less noble position in the electromotive series of elements. Theoretically, almost three times the capacity in ampere-hours is available per unit weight of magnesium compared with zinc, but this advantage is somewhat reduced in practice because of the dissolution of magnesium by parasitic corrosion during cell discharge. At very low discharge rates, the rate of parasitic corrosion of the magnesium can appears to be appreciably accelerated, resulting in an apparent loss of capacity.

A typical discharge curve for a magnesium cell is shown in fig. 5, with the initial voltage delay enlarged. The voltage is much more constant than that of the Leclanché cell on discharge.

Shelf-life is very good at room temperature and an average capacity loss of 10% per year can be obtained over three years for some cell types. Capacity loss at higher temperatures is much greater but is nevertheless better than for comparable Leclanché cell types. The performance at low temperatures is better than that of Leclanché cells.

The high voltage of the cells is a great space-saving advantage where high-voltage batteries are to be made. The main disadvantage of the cell is the high cost of the magnesium alloy and the magnesium bromide used in the electrolyte.

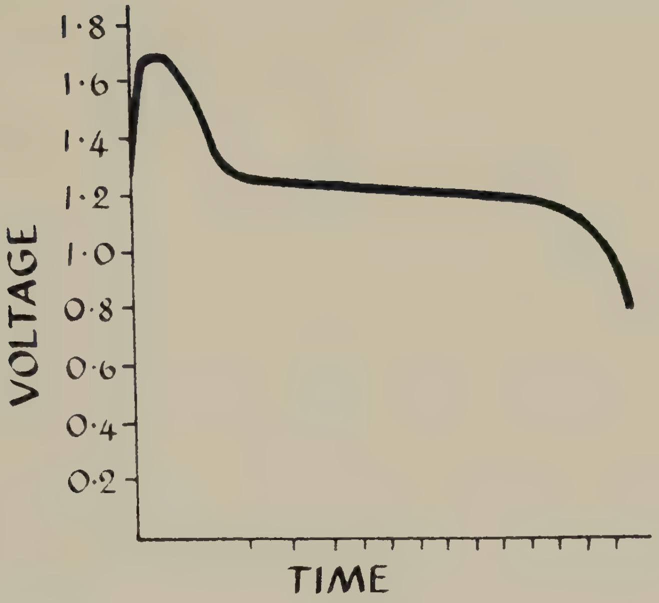

Fig. 5. Continuous discharge curve for a magnesium cell at room temperature.

4. The Ruben-Mallory (R.M.) cell

This cell utilizes mercuric oxide as a depolarizer and zinc as an anode material. The electrolyte is a 40% solution of potassium hydroxide containing sufficient zincate ions to prevent the dissolution of anode zinc.

Figure 6 is a diagram of a typical R.M. cell. The anode is a pressed powder cylinder formed from amalgamated zinc powder containing approximately 13% of mercury. The absorbent, which holds the electrolyte, is manufactured

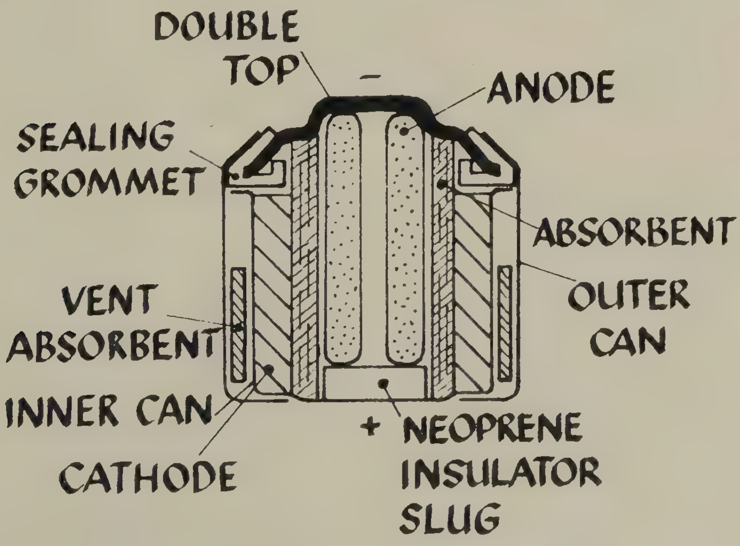

Fig. 6. Diagram of Ruben-Mallory (R.M.) cell.

from a type of porous paper and is spirally wound. A barrier, which is also of paper, is wound round the outside of the absorbent to prevent migration of particles of graphite from cathode to anode. The cathodic depolarizer is an intimate mixture of mercuric oxide and graphite containing approximately 7% graphite. This depolarizing cathode is also a pressed cylinder and is made to be a push fit to the inner can. The insulating grommet is moulded from polyethylene or neoprene. Inner can, outer can and outer top are nickel-plated steel; the inner top is tin-plated and provides the contact face with the anode zinc cylinder. The zinc cylinder is prevented from making contact with the base of the can by a neoprene insulator which also serves to maintain a pressure contact between the anode and inner top. Inner and outer tops are spot welded together providing an effective seal to the cell, when the outer can is radially crimped over the outer edge of the grommet, as shown in fig. 6. The inner can itself sits within the outer can shell, with the space between the two cans containing an absorbent paper. The system is remarkably stable and normally no gas is generated within the cell. If, under abnormal conditions, any gas is formed within the cell, the top lifts within the grommet allowing the gas to escape to the space between the two cans and from there through the opening at the cell base. Any entrained electrolyte carried into the inter-can space is soaked up in the vent absorbent paper.

The cell reaction of R.M. cells is as follows:

(1) $Zn + HgO + H_2O \rightarrow Zn(OH)_2 + Hg$.

(2) Reaction at the anode: $Zn - 2e \rightarrow Zn^{++}$
$Zn^{++} + 2OH^- \rightarrow Zn(OH)_2$.

(3) Reaction at the cathode:
$HgO + H_2O + 2e \rightarrow Hg + 2OH^-$.

The open-circuit voltage is approximately 1·35 V and the voltage falls quite rapidly to its equilibrium value when the cell is put on load. The efficiency of the cell is very high and under optimum conditions, almost 100% of the theoretical capacity is realised. R.M. cells are suitable for all current drains up to the maximum for which they were designed, whether continuous or intermittent. The cell is ‘ balanced ’ by slightly increasing the weight of mercuric oxide in the cathode beyond that required for complete oxidation of the anode zinc; in this way, no gas generation takes place if the cell is left on load when completely discharged.

Figure 7 shows a typical discharge curve for an R.M. cell; the flat discharge curve is a characteristic of the system. The cell case is manufactured largely from nickel-plated steel which gives a robust cell with a high corrosion resistance. Since the cells are hermetically sealed they are unaffected by pressure variation. Momentary short circuits will cause no permanent damage and the cell will recover in a few minutes. R.M. cells will operate at temperatures of 110°C for a short time and although the normal cell construction will not give efficient performances at low temperatures, a constant open-circuit voltage is exhibited under these conditions.

Another type of R.M. cell utilizes a wound anode structure of corrugated zinc foil; this type of cell is operable at both high and medium low temperatures

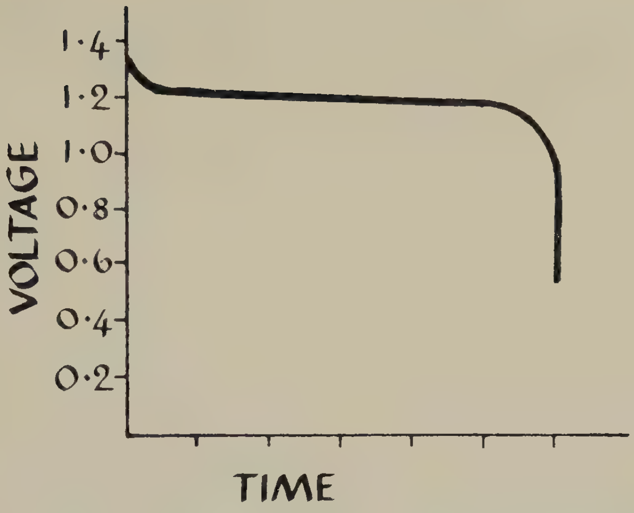

Fig. 7. Continuous discharge curve for an R.M. cell at room temperature.

down to 0°c. These cells can also be modified to produce high efficiency outputs at and below 0°c.

Shelf-life of R.M. cells at room temperature is usually two years and is determined by the attack of the mercuric oxide and electrolyte upon the cellulose within the cells. However, some cells have been successfully stored for over six years, giving over 90% capacity realization on a medium drain after this period of time. Shelf-life is curtailed at high temperatures, but even so the the stability is remarkably good.

5. Air depolarized cells

A form of air depolarized cell is shown in fig. 8. This particular cell has two wedge-shaped zinc anodes with a relatively massive porous carbon block

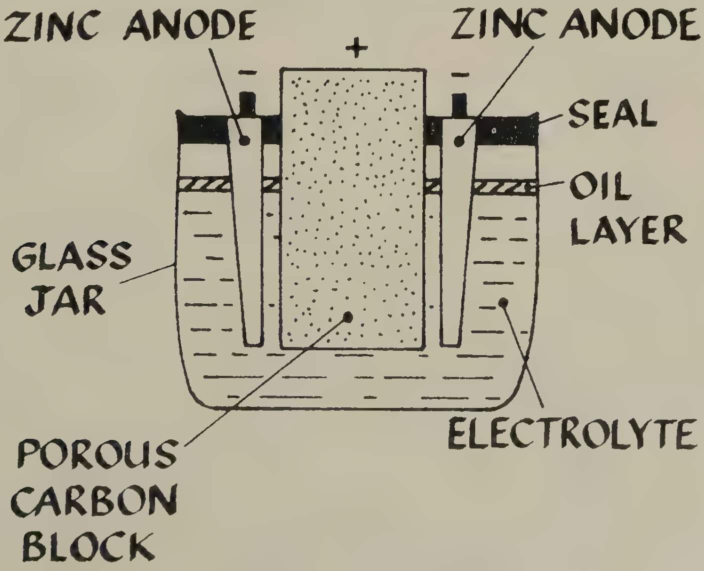

Fig. 8. An air depolarized cell.

between them. The electrolyte is a 20% solution of sodium hydroxide and the whole electrode assembly is contained in a suitable plastic or glass jar.

The overall cell reaction is as follows:

$$Zn+2NaOH+O \rightarrow Na_2ZnO_2+H_2O.$$

Anode reactions:

$$Zn-2e \rightarrow Zn^{++}; \quad Zn^{++}+4OH^- \rightarrow ZnO_2^{--}+2H_2O.$$

Cathode reaction:

$$H_2O+O+2e \rightarrow 2OH^-.$$

The sodium hydroxide in the electrolyte takes part in the cell reaction and according to this equation 1 g molecule is consumed per Faraday. In practice a larger quantity of sodium hydroxide must be allowed because a falling concentration of sodium hydroxide solution implies a rising internal resistance and this must be minimized. The anode product is soluble sodium zincate and in order to regenerate hydroxyl ions a quantity of calcium hydroxide is usually introduced into the electrolyte. Insoluble calcium zincate is precipitated with the re-formation of hydroxyl ions:

$$Ca\,(OH)_2+ZnO_2^{--} \rightarrow CaZnO_2+2OH^-.$$

In order to minimize open-circuit dissolution of the zinc anode, the zinc faces are heavily amalgamated and if the cell is to be stored for a long period prior to use, the electrolyte is only added when the cell is required.

Depolarization is effected by atmospheric oxygen which diffuses through the porous carbon block to the electrolyte/carbon interface; the shape and porosity of the block are critical if the cell is to maintain a required current density. In operation, the cell must be used in an adequately ventilated place with the upper face of the carbon block exposed to the atmosphere. Careful design of the carbon block is necessary to avoid nitrogen accumulation; a carbon to free-space ratio of 2 : 3 is desirable and the electrode must be porous enough to allow diffusion of air but fine grained enough to prevent electrolyte penetration. Carbonation of the electrolyte is prevented by floating an oil layer on the liquid surface.

The initial no-load voltage of this type of cell is about 1·46 v and the form of the discharge curve is similar to that of the Leclanché cell. Increased cell loadings can be obtained by operating the cell with pure oxygen passing through the cathode.

An air depolarized cell with a gelled electrolyte of the type used in the mercuric oxide cells has been developed, having an overall cell reaction independent of the sodium hydroxide content:

$$Zn+O+H_2O \longrightarrow Zn(OH)_2.$$

Air depolarized cells with ammonium chloride as the electrolyte have also been successfully produced.

6. CELLS STORED IN THE INACTIVE CONDITIONS

This type of cell is constructed without electrolyte, or without water, so that prolonged storage can be obtained. In most cases, cells manufactured in the 'dry' state have a virtually infinite storage life. There are three main types produced commercially: lead dioxide depolarizer with a neutral salt or acid electrolyte, chloride depolarizer with water activation and the dehydrated Leclanché type with water activation. All these cell types must be stored in a fully sealed condition to prevent moisture take-up from the atmosphere.

6.1. *Lead dioxide depolarized cells*

Metals used with acid electrolytes are zinc, cadmium or lead, the cathode reactions are of this form:

$$PbO_2 + 4H^+ + 2e \rightarrow Pb^{++} + 2H_2O.$$

Zinc and cadmium cells have a sulphuric acid electrolyte and the overall reaction for a zinc/lead dioxide cell is:

$$PbO_2 + Zn + 2H_2SO_4 \rightarrow PbSO_4 + ZnSO_4 + 2H_2O.$$

Lead/lead dioxide cells usually have a perchloric acid electrolyte:

$$PbO_2 + 4HClO_4 + Pb \rightarrow 2Pb(ClO_4)_2 + 2H_2O.$$

The no-load voltages of the cells are pH dependent, but for the same acid strength the open circuit voltage of the cadmium cell is slightly lower than that of the zinc cell. Addition of mercuric sulphate to the electrolyte is necessary to prevent local action, which it does by amalgamating the zinc.

Another lead dioxide cell has a potassium chloride electrolyte with a magnesium anode. This cell is made with an absorbent impregnated with potassium chloride and is activated by the addition of water only.

All these types of cell are ideal where long storage life followed by a relatively short discharge period is required. They give satisfactory operation over a wide temperature range, but for optimum storage and discharge conditions, the cadmium cell is preferred. The cells generally take the form of flat plate electrodes, with a separator; at normal temperatures, activation is rapid and is usually less than one minute, depending on the cell design.

6.2. *Chloride depolarized cells*

These cells are depolarized either by silver chloride or cuprous chloride and activated by sea water, or by incorporating a solid chloride in the battery which is then activated by fresh water. The anode is usually magnesium because of the high output voltage obtained with this metal, which may be either a flat plate or a strip. When cuprous chloride is used as the depolarizer, the powder is packed round or pressed onto a piece of copper gauze. Silver chloride is much easier to fabricate because of its ductile and malleable nature; it may be rolled into sheets or pressed into blocks and connection to the electrode is made by silver strip or wire. Cells can be of the flat plate or rolled type, the latter having a structure like an electrolytic condenser, with a highly absorbent separator between anode and cathode.

The reactions taking place within the unit are the complete solution of the anode metal as the chloride and the reduction of the cathode to the metal. The conductivity of the electrolyte and the cathode will therefore increase with the cell discharge. During discharge heat is generated within the cell and this useful property is a great advantage when the cell is to be used at low temperatures. The open-circuit voltage of the silver chloride/magnesium cell is about 1·5 and of the cuprous chloride/magnesium cell about 1·35. Zinc may also be used as an anode metal, but no heat generation takes place on discharge and the no-load voltage is of the order of 0·85 in this case.

This type of cell is suitable for long term storage followed by a relatively short discharge on medium–heavy current drain. For optimum performance and shelf-life the magnesium/silver chloride cell seems to be the best choice. Activation time for silver chloride cells is very short, being normally less than one second, depending on the structure.

6.3. *The dehydrated Leclanché cell*

The Leclanché cell is difficult to store in tropical or desert conditions and work carried out in Australia proved that it was possible to dehydrate a freshly made Leclanché ' dry ' cell, then to reactivate it after storage. The Leclanché cells used were of a type with an enclosed depolarizer block, encased in muslin or surrounded by a paper layer. The cells were left unsealed after assembly, then sealed in an autoclave. Dehydration was carried out in two stages: first 48 hours at 50°C in a partial vacuum followed by 48 hours at 50°C under high vacuum. Cells were assembled into a P.V.C. web, connected in series to form H.T. batteries and finally sealed off in a polythene outer case. This battery was reactivated with water after various periods of storage under tropical conditions and gave far better performance than conventionally manufactured H.T. batteries under the same conditions of storage.

7. Nuclear cells

This type of cell is really a radiation convertor and does not strictly fall into the class of primary cells. However, the cells are primary in the sense that they are not re-chargeable and a brief description of some of these cells would not be out of place here.

A simple type of nuclear battery was produced by Moseley over forty years ago; this cell consisted of two electrodes, one of which was coated with a β-active material and the β-particles emitted from this electrode were collected at the other electrode, thus allowing the current to flow in an external circuit. This cell has been further developed in recent years by placing a dielectric (e.g. polyethylene or polystyrene) between the electrodes, which allows the penetration of high-energy electrons to the collector, but prevents the passage of low-energy electrons in the opposite direction. It is obvious from this description that the construction is virtually that of a capacitor with one plate activated. The β-active isotope most frequently used is strontium 90 and this type of cell gives a high-voltage/very low current output.

Another type of nuclear cell using a β-active isotope consists of two electrodes, the materials of which have very different energy requirements for the release or orbital electrons, e.g. zinc and carbon have been used, separated by a gas which is usually tritium. The gas between the electrodes is ionized by the radiation from the isotope; positive ions and electrons carry the current between the electrodes. Current amplification takes place within this cell due to avalanche ionization, triggered by the β-particles. Current is still very small and the e.m.f. of the cell is of the order of 1 V.

The p–n semiconductor junction has also been converted for use as a nuclear cell; injection of β-particles produces a flow of electrons across the junction.

Current amplification is high in this cell and although currents are many times greater than those available from the previously mentioned nuclear cells, they are still only of use with low power devices. The e.m.f. of the cell is about 0.2 V. The use of strontium 90 as a source of β-particles is avoided where possible because the maximum energy of β-particles from this source is high enough to cause damage to the semiconductors. Maximum β-particle energy is preferably about 0.2 MeV for optimum operation.

Less direct conversions of radiation energy to electrical energy have also been investigated. One method is to allow radioactive particles to fall on a scintillator and use the light output to eject electrons from a p–n semiconductor junction. The other method is to use an α-particle emitter to generate heat in a section of material connected to a thermal junction, which will then produce a small current.

All nuclear cells have the disadvantage of producing low power outputs, but they can be made to give very long discharge lives with good reliability. The choice of isotopes is limited because of the danger of γ-radiation which would necessitate massive shielding devices.

8. FUEL CELLS

This type of cell is not generally treated as a primary cell, although the principles of operation are similar; fuel cells are continuous converters of chemical to electrical energy and like nuclear cells, they are not designed to be rechargeable.

The direct conversion of fuel to electrical energy is an attractive proposition. The low efficiency stage of the steam boiler is completely eliminated, leaving the possibility of a direct high efficiency process. Much work has been done on many types of fuel cell systems but only the Davtyan water-gas cell with its modifications and the Bacon cell will be dealt with here.

The original Davtyan cell utilized an oxygen (air) electrode and a water-gas (1 : 1 mixture of carbon monoxide and hydrogen) electrode with a solid electrolyte which was basically sodium carbonate. The operating temperature of the system was about 700°C and the no-load voltage about 1 V. The basic overall cell reaction was

$$CO + H_2 + O_2 \longrightarrow H_2O + CO_2$$

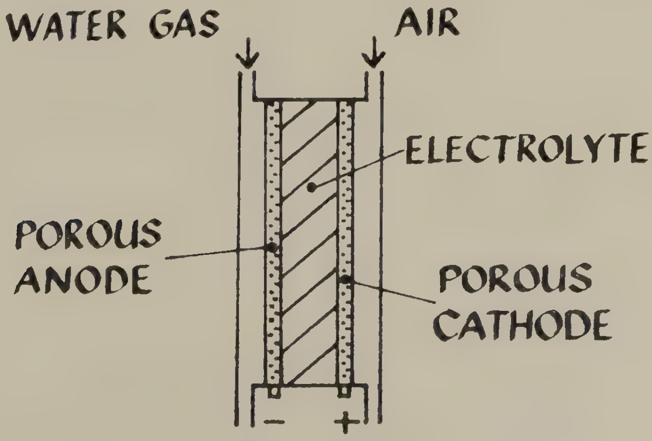

Fig. 9. Principle of the Davtyan fuel cell.

A simple diagram of the Davtyan cell is shown in fig. 9. Both electrodes in this cell were iron/iron oxide types. More recent work in this country has resulted in a much improved cell operating on water-gas and air, with an operating temperature reduced to 600 °C. The electrodes are manufactured from silverized zinc oxide and the electrolyte is a eutectic mixture of sodium and lithium carbonates, which is maintained in a stable condition by feeding carbon dioxide in with the air. The ‘ available energy ’ to ‘ electrical energy ’ conversion can be raised to above 95% by operating cells in banks and feeding the gases in cascade through several electrically inter-connected cells. The operating gas pressures in these cells are slightly above atmospheric pressure: these waste gases emerge from the cells at above 500 °C and further energy may be usefully removed by heat exchangers to improve the overall efficiency. These cells do not suffer appreciably from either activation or concentration polarization below current densities of 160 mA/sq. cm; up to this figure the voltage drop is mainly due to internal ohmic resistance. Banks of these cells will operate at approximately twice the current density of the original Davtyan cell for the same voltage drop.

The use of the hydrogen–oxygen reaction to obtain electrical energy has been known since the experiments of Grove, carried out in the early nineteenth century. Both Davtyan and Bacon have worked on this type of fuel cell and Bacon's work has resulted in a practical fuel cell power unit which has been recently demonstrated.

The cell reaction when 4 Faradays of electricity circulate is:

$$2H_2 + O_2 \longrightarrow 2H_2O.$$

The overall anodic reaction:

$$2H_2 + 4OH^- - 4e \longrightarrow 4H_2O.$$

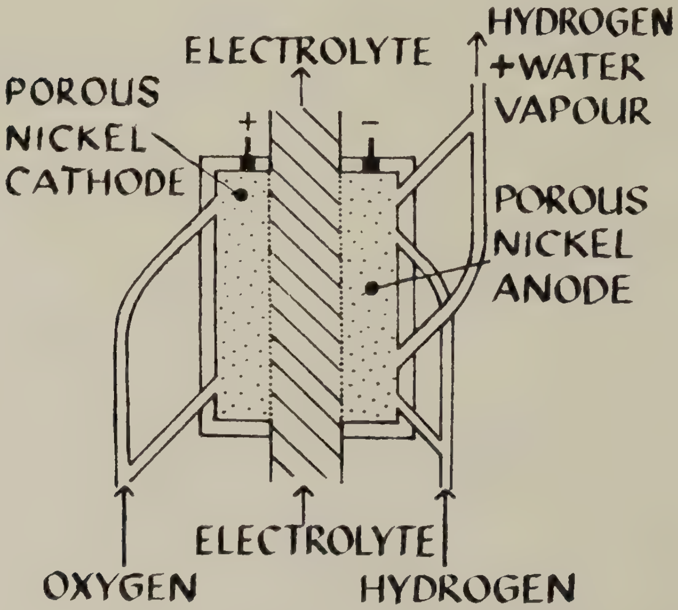

Fig. 10. Principle of the Bacon fuel cell.

Overall cathodic reaction:

$$O_2+2H_2O+4e \longrightarrow 4OH^-.$$

A simple diagram of the Bacon cell is shown in fig. 10.

Both electrodes are of porous nickel, the cathode being specially treated to prevent corrosion during cell operation. Oxygen is forced through the porous nickel cathode and hydrogen through the anode, this hydrogen is simultaneously drawn off and re-circulated through a condenser to remove water vapour. The electrolyte is approx. 30% aqueous caustic potash solution, which is circulated by convection to reduce concentration polarization. The electrodes are separated by the minimum possible distance and the faces in contact with the electrolyte have a very fine grained nickel layer to prevent the gases bubbling into the solution. Operating temperature is about 200°C, with gas pressures around 500 pounds weight per square inch. The performance of this cell is particularly good; at a current density of 250 mA per geometrical sq. cm of electrode the voltage falls from 1 V to 0·8 V. Since the minimum performance expected from a fuel cell should be 100 mA/ geometrical sq. cm at 1 V, this performance shows up very well.

This cell may find further application in the storage of electrical energy by using it to decompose water to hydrogen and oxygen in high pressure electrolysis equipment; these gases can then be reconverted to electrical energy at will, in the Bacon fuel cell.

9. CONCLUSION

The increasing trend towards miniaturization in electronic equipment, accompanied by the demand for miniature cells with increased storage and discharge life, provides added incentive to the still expanding industry of primary cell manufacture. A great deal of work on the development of new electrochemical systems is being carried out and some of the cell systems discarded as impracticable years ago are being subjected to rescrutiny, with a view to possible utilization in primary cells. Over the past few years, the contribution of primary cells towards scientific advancement has been a significant one and there is every indication that this state of affairs will continue in the future.

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The 44th Physical Society Exhibition, 1960

(1) A visitor's impressions

by W. SIDDALL

The 1960 Physical Society Exhibition (the 44th) was held in the Halls of the Royal Horticultural Society, Westminster, from 18 January to 22 January, and was opened by Mr. A. J. Philpot in the absence through illness of the President of the Society, Mr. J. A. Ratcliffe, C.B.E., M.A., F.R.S., whose future appointment as Director of the Radio Research Station had just been announced.

Electronic apparatus, ever keeping up with the times, is approaching "do-it-yourself" independence. That recent introduction, the digital valve, is becoming more popular; the range has been increased by valves portraying simple fractions, addition and minus signs. Sixteen firms exhibited digital indicators. One wonders whether it is necessary to make life so easy for the technician. Surely the introduction of such thermionic devices is likely to lower the reliability and to increase the probable error of measuring instruments; a technician should be able to estimate the position of a pointer on a scale. If it is desirable to eliminate the human factor, an instrument to print the numerical answer as a permanent record, and to give a positive warning when a limit has been reached would be much more useful. For over two years one has expected a prototype model which bellows the answer; it should not be difficult to do.

Apparatus continues to increase in size, weight and complexity; the smaller the parameter to be measured, the larger the apparatus required. On the other hand, apparatus that is intended to be portable has become more compact. There were several examples of small, neat C.R.O.s on show; also a portable potentiometric recorder using transistor techniques for the self-balancing recording mechanism (Mervyn Instruments). Researches in transistor circuiting are directed towards microminiaturisation; two notable exhibits were circuits deposited on glass (Royal Radar Establishment), and semi-conductor solid circuits formed by processes such as diffusion, deposition, and alloying in small single crystals (Texas Intruments).

A number of exhibitors showed applications of moiré fringes now used for very accurate measurement of minute displacements in the machine tool industry. Examples of zone refining were on show. In one case, large scale apparatus for producing very pure gallium arsenide for use as a semiconductor (Services Electronics Research Laboratories), and a semi-micro version (Baird & Tatlock).

Six exhibits by Swiss instrument manufacturers were specially arranged by the Swiss Physical Society and the Swiss Office for the Development of Trade. A model of a 4 MeV ion accelerator in a pressuriezd tank for nuclear research (Haefely & Co.) was impressive; and we saw the versatility of the Metrohm Potentiograph E. 336 for the automatic recording of titration curves. It was a very good example of Swiss workmanship.

The stand of Associated Electrical Industries showed the wide range of activities by the previously separate companies, now designated as divisions. One example was the Birlec method of drying gases by molecular sieves. An analogue multiplying unit using the Hall effect and transistorized amplifiers was well presented by the 'Metro-Vick' division.

Another major stand, that of the Department of Scientific and Industrial Research, grouped together displays by fifteen laboratories. The Wool Industries Research Association showed an assembly for the automatic chromatography and assay of radio-active substances.

The Science Museum contributed two working models, showing the principle of the cyclotron and of the cascade generator.

Several well-known firms were not exhibiting, since thirteen firms volunteered to absent themselves in order to relieve pressure on space. This alone indicates the popularity of the Physical Society's Exhibition among the exhibitors; attendance on open days shows its appeal to the scientifically minded public.

(2) The Physical Society Colour Group's Exhibition

by M. H. WILSON

The appearance of colours is determined not only by their spectral composition but also to a great extent by the influence of other colours perceived at the same time or in the immediate past. These effects, which take place within the visual process itself, are known as Adaptation.

Demonstrations of Colour Adaptation were arranged in sequence, the earlier ones being devoted mostly to Surface Colours. Simultaneous Contrast was shown with coloured papers seen against differently coloured backgrounds, and also with a series of light grey surfaces in a dark surround, each one in turn being judged as white when it is the lightest surface in the field of view. Another device showed how a bright yellow card, when the illumination upon it is lowered relative to its white surround, is seen as dull olive or brown. Successive Contrast was illustrated by the familiar Bidwell's Disc, in which the appearance of a red lamp is obliterated by its own blue-green after-image by means of a sectored white disc rotating at the appropriate speed; and also by the variety of changes in the appearance of coloured papers when a coloured after-image is projected upon them. A less familiar effect, occurring in closely spaced patterns, in which colours are changed *towards* the colour of their surroundings was also shown.

A further example of Adaptation, due to macular pigmentation was made apparent by shifting the gaze to and fro between pairs of coloured samples of similar hue but different spectral composition.

Simultaneous Contrast by projection with coloured light sources was illustrated by the experiment with shadows thrown from one coloured and one colourless source, as described at length by Goethe (*Farbenlehre*, 1810); also by the projection of an apparently fully coloured picture (after Van Gogh) from three projectors using *blue filters only*, as demonstrated two years ago at a Physical Society Exhibition Discourse; and again by a coloured picture from two photographic separation-positives projected with red light and white light only, as described recently by E. H. Land.

Sets of colour-transparencies were used to show the wide variation of overall colour to which the eye can comfortably adapt, but that this adaptation cannot compensate for the effects of exposing daylight-type colour film in tungsten light and vice versa. The last group of exhibits was concerned more directly with colorimetry, and showed that the validity of a match established between the two halves of a bipartite field is independent of the colour of the surrounding field only if the latter is uniform; that the presence of the retinal 'yellow spot' can cause a discrepancy between the results of using large and small viewing fields, and that for the measurement of coloured samples the angles of illumination and of viewing must be standardized. The three internationally accepted Standard Illuminants were shown, and also a method for measuring the subjective appearance of identical samples seen under different conditions of illumination. The final exhibit was a diagrammatic representation of a modern colorimeter.

The Exhibition was designed to show the many factors which have to be taken into account in any workable system of colorimetry. Effects which at one time were regarded as optical illusions are now seen to contain essential principles of vision.

(3) Atomic clocks

by L. ESSEN

A short account of a Discourse given at the Exhibition

The development of the atomic clock has provided a unit of time interval which is far more accurate and more readily accessible than that obtained from astronomical measurements. The mean solar day is believed to have varied by about 0·007 sec (i.e. approximately 7 parts in 10^8) during the past two centuries and is known to have an annual periodic variation of about 1 part in 10^8 in addition to inexplicable irregular variations. An alternative astronomical unit known as the second of Ephemeris Time based on the revolution of the earth about the sun has been adopted by international agreement; but it has the grave disadvantage that it cannot be measured very precisely. The observations must be extended over many years to give the accuracy required and the value can be given only in retrospect.

The physicist and radio engineer has therefore turned to the natural periodicities of the atom for his standard of time interval. Any spectral line, having a frequency given by the Bohr relationship $f=(E_2-E_1)\,h$ where E_1, E_2 are two different energy levels of the atom, and h is Planck's constant, constitutes a potential standard of frequency and time interval; but very few lines answer the practical requirements. These are that the frequency must be in the range which can be generated in the laboratory, that it should be independent of external conditions, and should be capable of observation with great precision. Of the few possible lines the one resulting from transitions between the two ground state levels of the caesium atom has marked practical advantages, the frequency near 9192 Mc/s being very suitable from the electronic point of view, and the caesium atom having magnetic properties which enable a very precise method of detection to be employed. This is the atomic beam magnetic resonance method developed by Rabi and his co-workers at Columbia University.

A thin pencil of atoms travels a distance of about 1 m in a highly evacuated pipe and the atoms are deflected away out of their path by a system of two strong magnets. If in the space between the two magnets the atoms pass through a very small magnetic field alternating at the Bohr frequency, the atoms resonate, reverse their magnetic polarity and return to a detector on the original line of the beam. The oscillator used to excite the alternating field can thus be set to equality with the atomic frequency; and in turn can be used to calibrate the working standards of frequency, which are usually quartz clocks.

The setting can be made with great precision, so that the quartz clocks can be calibrated in terms of the atomic frequency with an accuracy of 1 part in 10^{10} in a measurement extending over a few minutes. The speed of measurement enabled controlled experiments to be made at the National Physical Laboratory on the effect of varying the various design parameters. These showed that under correct conditions of operation the only two parameters affecting the frequency significantly were the strength of the d.c. uniform field in which the transitions occurred and the phase of the alternating magnetic field. Both of these parameters can be checked experimentally by the equipment itself with sufficient accuracy to ensure that the frequency is within 1 part in 10^{10} of the true atomic frequency at zero field.

Exhaustive comparisons have also been made with the Atomichron, developed independently in the U.S.A. and differing in many respects from the N.P.L. standard although based on the same spectral line. The frequencies are in general found to agree to 2 parts in 10^{10} and as some of these comparisons were made by means of radio transmissions across the Atlantic the results show that the atomic standards can be used in practical applications with this accuracy.

A series of measurements made in collaboration with the U.S. Naval Observatory enabled regular comparisons to be made between the N.P.L. standard and astronomical time. These results have given an accurate measure of the variations in the speed of rotation of the earth and have also furnished the relationship between the atomic standard and the second of Ephemeris Time. The atomic standard can thus be used to make this unit immediately available. The close co-operation of physicists and astronomers has prevented the confusion which would have been caused by the introduction of an uncorrelated atomic unit and should enable full advantage to be taken of both the precision of the atomic standard, and the continuity of the astronomical standard.

(4) Recent advances in solid-state physics

by D. A. WRIGHT

A short account of a Discourse given at the Exhibition

Recent advances in solid-state physics owe a great deal to improved techniques and the wider application of techniques which were at a research stage not long ago. Examples of the former are the development of methods for increasing the purity and perfection of crystals, for producing very large magnetic fields, or for improving resolution in optical equipment; examples of the latter are the rapid expansions in the use of liquid helium temperatures, micro waves and very large pressures. In the meantime solid-state theory has advanced and is able to correlate many of the observed properties of the bulk materials. The behaviour of surfaces is much less thoroughly understood, though some progress has been made.

In parallel with the general advance in the subject, new devices have been developed and existing ones have been improved. Thus, there have been improvements in the performance of semiconductor diodes and transistors as regards frequency, power, and stability, and an extension of the temperature range over which they will operate; the solid-state analogue of the thyratron has been developed, and new types of two-electrode structure have appeared, resulting in low noise-level amplification, and in higher frequency operation. The understanding and control of photoconducting materials has improved considerably, and there has been some progress in phosphors and electroluminescence. Switching and storage devices operating at helium temperatures are rapidly being perfected, while the ' maser ', a new type of microwave amplifier, has developed rapidly after its relatively recent invention. This also operates at very low temperatures. The development of new semiconducting compounds has resulted in great improvement in the performance of thermoelectric devices, both for refrigeration and for generation of electricity.

The points illustrated by demonstrations were: The increase in the maximum frequency response of transistors, making possible a fully transistorized F.M. receiver which was demonstrated and compared with a medium-wave A.M. transistor set; methods for automatic control of lighting installations, using firstly a cadmium sulphide photoconducting cell and secondly a silicon p–n–p–n controlled rectifier; the type of ruby crystal used in masers; and experimental thermoelectric cooling and generator units, both employing bismuth telluride alloys.

Slides were used to illustrate parametric diode amplifiers, cadmium sulphide photovoltaic cells and several aspects of the progress in fundamental knowledge of solid-state physics.

The Physical Science Study Committee
(2) The Summer Institute courses

by HENRY F. BOULIND
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In the summer of 1959 the author, together with a small number of persons from other European countries, attended Summer Institutes in the United States. The visits were arranged by the Organization for European Economic Co-operation (OEEC) and were financed by the National Science Foundation of America. These Summer Institutes, held at about twenty University centres in various parts of the United States, were organized by the Physical Science Study Committee.

This article deals with (1) the aims of the Physical Science Study Committee, (2) the author's experiences at the PSSC course at Portland, Oregon, (3) an attempted appraisal of the results the PSSC course may be expected to produce, (4) some suggestions concerning the possible usefulness of PSSC research in this country.

1. Aims of P.S.S.C.

" Since the early years of the 20th century ", says E. P. Little, Executive Director of the Physical Science Study Committee, " the natural sciences have undergone two distinct and consequential changes. First the sciences themselves have grown enormously. . . . Next, science has become inextricably woven with our daily life."

Yet, in spite of this, the structure of school science teaching has remained substantially the same as it was fifty years ago. The problem is seen most clearly by considering the school Physics syllabus. It is built round Newtonian mechanics. The universe, as the physicist then saw it, was a Newtonian universe. Accordingly the course began with statics, went on to kinematics and dynamics, and in the light of these disciplines, undertook to explain heat, light and sound. There was a logical unity, and the course reflected the current state of knowledge and the general attitude of physicists.

In the present century, Newtonian mechanics has changed in status rather than lost in significance; it is no longer the only manner in which the physicist regards his universe. Quantum theory and relativity were developed, wave mechanics came into being, and attention shifted from the molecule to the atom, from the atom to the nucleus. Yet, however great these changes, they are overshadowed by what, to the casual view at any rate, seems an even greater change—the growth of technology. Since both could not be covered in the time at a teacher's disposal, the tendency has been to shift the emphasis from science to technology: to keep the nineteenth-century syllabus but to add to it bits and pieces about the working of an automobile, or a radio, or a refrigerator, or a space ship. Apart from these addenda, school physics is not living and advancing physics. The Physical Science Study Committee was organized in 1956, under a grant from the National Science Foundation, to seek ways of giving expression to a modern outlook in physics teaching. The initial impulse came, not (as it has in this country) from school teachers themselves, but from a group of professors at the Massachusetts Institute of Technology. The Committee is administered by the MIT Department of Sponsored Research, but it is by no means an exclusively MIT project. According to Professor F. L. Friedman, of MIT, the specific aims of the Study Committee are:

(1) to plan a course of study in which major developments of physics, up to the present time, are presented as a logical and integrated whole.

(2) to present physics as an intellectual and cultural pursuit which is part of present-day human activity and achievement,

(3) to assist physics teachers, by means of various teaching aids, to carry out the proposed programme.

Something approaching $2 million has already been spent on the project. The Committee has permanent headquarters comprising offices, laboratories, work rooms and film studios at Watertown and Cambridge, Massachusetts. Its productions include five textbooks covering the new syllabus (see Mr. J. L. Lewis's article, *Contemporary Physics*, Vol. 1, No. 3, Feb. 1960), laboratory manuals, teachers' guides and notes, and a special series of monographs. Simple and inexpensive apparatus has been designed—including such devices as a ripple tank, or cloud-chamber, and the parts are on sale at cheap prices. Films and filmstrips have been produced. And—most necessary of all—summer courses are arranged at many colleges from Brunswick, Maine, to Los Angeles.

2. SUMMER INSTITUTE AT PORTLAND

Daylight on 20 July 1959 lasted 29 hours; this was certainly the oddest day I have ever spent! My plane left Heath Row a few minutes before midnight on Sunday, and by the time we reached the north of Scotland, Monday's dawn began to show. At 4 a.m., local time, we touched down for half an hour on Baffin Island in the Canadian Arctic. We reached Seattle at 9 a.m., Pacific time, having crossed the Rockies, then I caught the coach-plane to Portland, arriving at my destination, Reed College, in time to get unpacked and settled in before lunch. On Monday afternoon I was working in the laboratory, then I had a swim (it was hotter in Portland than in London even—Baffin Island had seemed beautifully cool), and the day finished (suitably) with a lecture on Relativity! This lecture was very ably given by my European co-observer on the course, the headmaster of the Royal Grammar School in Copenhagen.

My reactions to the sudden time-shift were interesting. It took several days for me to catch up—I would wake early and, when rising at 7.30, it seemed as if I had been in bed till midday; then, of course, I was sleepy and ready for bed about 7 p.m.

I stayed at Portland for only three weeks of the eight weeks' course. Imagine being able to arrange an 8 weeks' course for English science teachers—and there were still 6 or 7 weeks of holiday left! This pinpoints one trouble about English Grammar School teaching: too short summer vacations. Saturdays and Sundays were completely free. Mondays to Fridays were very full: lecture at 8.30 a.m. followed by discussion, coffee break, talks and discussion on, first, laboratory work for the afternoon, then, last night's and next night's ' homework'.

This was followed by ' seminars ' in classes of about 10. The afternoon was occupied with making and using apparatus, then there was some free time, followed, on most evenings, with lectures, or films designed for the PSSC syllabus. The total numbers on the course were 38 men and two women. This, I gathered, just about represented the ratio of men teaching physics to women; the disproportion does not in itself matter very much since almost all U.S. schools are co-educational, The *overall* shortage of science teachers is even worse there than it is here.

The staff of the course comprised Professor Davis and Dr. Youtz of Reed College Department of Physics, and two high school teachers, Mr. Walter of Tacoma, Washington, and Mr. Alexander, of Claremont, California. These last two had had experience of teaching the PSSC syllabus in their schools. All four, together with staff members of the other Summer Institutes in progress, had attended a week's course of preparatory work and discussion in Massachusetts in June, just before the Summer Institutes began. An important point is the interest the U.S. *universities* now take in what goes on in the schools. This leads to a subject-centred rather than a child-centred approach. ' Child-centred ' seems, in these courses at any rate, to be a rather naughty word: Americans are ' Dewey-eyed ' no longer.

They were a wonderful and hospitable set of people to be with. What did they get from the course? I formed the opinion that their general level of knowledge of physics was, perhaps, somewhat less than that of a corresponding group in this country, and that the main thing they acquired was knowledge of *Physics* rather than of how to teach it. But they were certainly enthusiastic—and this was at the end of the course, not the beginning!

It was in a large lodge half way up Mount Hood that I met a party of chemists who were taking a weekend off from the committee work of preparing syllabuses, etc., in Chemistry, to some extent parallel to the PSSC courses in Physics. They were most despondent about American education for more able pupils, and all that I said about English schools only increased their despondency. I had to point out (as happened so often in discussions) that, while we may be doing a better job than the Americans for the top 20 per cent. there wasn't much doubt that they achieve more for the lower 80 per cent.

I reluctantly left my new friends in Portland, flew south to stay for a few days near Los Angeles, and then went to Boston and Cambridge where I saw something of Harvard MIT, and also PSSC headquarters, including the film studio where a teaching film on electrolysis was being shot.

3. IMPRESSIONS AND DISCUSSION

I cannot make any really valid appraisal because (*a*) that could only be done in the schools, which of course were shut at the time of my visit, and (*b*) in any case, the scheme was only just reaching its conclusion—and then ' feed-back ' of criticisms and new ideas from the schools will be necessary. However, one thing seemed obvious: the syllabus is too long—grotesquely too long by our standards—to be covered in the one-year's course, for which it is designed. One year, that is, with pupils of 16+ years of age. It is rather like trying to cover all Physics for ' A ' level in one sixth form year, *starting with no previous knowledge*, and having no more time than is allowed in English schools for a

Physics specialist. However, the Study Committee—correctly to my mind—have paid more attention to *ideals* than to present practice: they have found what has to be done and now they are setting about getting the time to do it. I understand that the Committee will campaign for (*a*) two years of Physics instead of one, (*b*) some ' real Physics ' to be done before the age of 16. Even so, I doubt whether the last portion of the syllabus can be understood and appreciated by the 25 per cent. of the school population at which it is aimed.

(The following are the section headings of Book 4 Chapter 8: *Quantum Systems and the Structure of Matter:*

The Experiments of Franck and Hertz; Atomic Energy Levels; Excitation and Emission; The Energy Levels of Hydrogen; The Origin of Energy Levels; The Wave Theory of Hydrogen Energy Levels.)

Nevertheless the new scheme is a source of inspiration, it is ' living Physics ', an excellent introduction for the future specialist, and a fine general education for all. If it is generally adopted and made to work, the Americans will go a long way towards overhauling the big lead in school science education which, by all accounts, the Russians seem to have established.

The group of pupils in Britain, comparable to that for which PSSC is designed is, of course, the *whole* of the Grammar School-Public School population. None of our young people is likely to be getting a course of Physics as good as that set forth in the PSSC text-books.

However, a new policy has been set forth by the Science Masters' Association in the statement ' Science and Education ', and new syllabuses at ' O ' and 'A' level are at present being worked out by SMA panels. If these were adopted, then our teaching could be, in theory, just as good, or better than, the Americans', and would stand much more chance of being effectively put into practice. Our aims, as regards real, fundamental, living Physics are identical; the difference is that the PSSC is spending half a million dollars a year, and the SMA is not! We could profit by American expenditure. My suggestion is: *Send five more Physics teachers to the Summer Institutes in* 1960. Then we who have participated might get together, and decide to what extent, and by what methods, American research and experience can be applied to British conditions. Also, we hope to persuade Professor Friedman, or some others of the PSSC top personnel, to lecture and demonstrate on this side of the Atlantic.

The cost? About £400 per person we send across. £2000 very well spent. Might some organization having the teaching of science at heart provide it? Or part of it?

The postgraduate training of physicists in British universities

THE REPORT OF A JOINT COMMITTEE OF THE INSTITUTE OF PHYSICS AND THE PHYSICAL SOCIETY, MARCH 1960

The committee set up by these two bodies under the chairmanship of Prof. N. Kemmer, F.R.S., heard the views of representatives of universities, industrial employers, and government departments on the present system of postgraduate training for the Ph.D. degree. The industrial view is one of disappointment with results, expressed in terms so strong as to suggest that a doctor in the works is the equivalent of a spanner. The universities recognize the need for further formal tuition after the first degree, as is the custom in some other countries.

The views of recent students themselves do not seem to have been canvassed, but they must often feel themselves to be the centre of competing interests. Universities need strong research schools—though the junior members of a large research team may be mere participants. Industry recruits Ph.D.s as pearl fishermen collect oysters—though it would prefer to have them three years earlier and be more hopeful of getting pearls cultured by itself. In the most unfavourable combination of circumstances it would seem possible for a man to spend three unprofitable years acquiring a degree that is itself a dead loss.

Firms, university departments, and students vary so much that generalization is difficult; but the following conclusions are constructive. And the suggestions they make all seem to be entirely in the best interests of the third but equally important party to them all, the student himself. The committee concludes:

1. The Ph.D. is an admirable training for subsequent research, and for men of originality and high intellectual ability, it can produce technical accomplishments and the right mental attitude for successful fundamental research in industrial or other laboratories. If it is to retain its value as a hallmark of capacity for original research, the universities must be jealous of the high standard of those on whom it is conferred.

2. The research student plays an important part in maintaining the vitality of a university physics department. The universities, however, must not seek expansion of research activity without limit and they must ensure, which is not always the case at present, that research students are not employed partly as high-grade technicians.

3. The standard of the Ph.D. in physics is at least as high as in most other subjects but the degree is sometimes (and, in the opinion of some employers, often) awarded to candidates of inadequate calibre. The universities should be more critical in their selection of research students and more willing to refuse the award, even after the completion of the prescribed period of study and research. There is a strongly held opinion among employers that the standard of the Ph.D. has deteriorated significantly in the past 20 years; the remedy for such an impression lies entirely within the power of the universities.

4. Industry needs physics graduates of wide knowledge, adaptability and first-class intellectual capacity and would prefer that only a small proportion of its recruits had trained for a Ph.D. in a university. Comparatively few jobs

in industry will involve research in pure physics and students who contemplate staying at the university for a Ph.D. ought to appreciate that the degree will not usually improve their prospects of advancement in industry.

5. Some university teachers consider that industry's lack of enthusiasm for the Ph.D. arises from the fact that, in physics, fundamental research is very largely carried out in universities. It would help if more members of university staffs were aware of the problems of industry and the requirements of industrial research and more industrialists appreciated the crucial place of research and the research student in the modern university.

6. An extension of knowledge, beyond that appropriate to the present first degree courses, should form part of university training at postgraduate level. The average postgraduate student is not capable of achieving this extension in his own time and on his own initiative, and postgraduate teaching of a formal character is desirable for at least the great majority of research students. For these students there is a strong case for the concentration of this teaching into the first postgraduate year.

7. Many graduates who enter industry without taking a Ph.D. would benefit greatly from courses of advanced lectures similar to those which the research student ought to take. To organize these courses so that they could be available to both groups of students would meet a real industrial need, might ease the problem of organization within the universities and might help in the assessment of students capable of staying on for research. Further, courses of this kind might stimulate vitality and generally strengthen some university departments.

8. The universities are faced with serious problems of staffing and the development of postgraduate courses would aggravate these. This is a matter to which the University Grants Committee would need to give sympathetic consideration.

9. The great majority of university students—undergraduate and postgraduate—are dependent on maintenance grants. No improvement of training along the lines suggested, involving for the best students at least an additional year at the university, is feasible unless the grant-awarding bodies, particularly the Department of Scientific and Industrial Research, are prepared to provide the necessary financial support.

10. The report is only a first step in investigating a very big problem. It has concentrated almost exclusively on problems arising in the training of those working for the Ph.D. degree but recognizes that there are at least equally important problems involved in the further training of those who will take up professional work in physics immediately after taking a first degree. An attempt to investigate such a subject would involve consideration of the nature and extent of industry's need of scientific manpower.

The Report is obtainable from the Institute of Physics, 47 Belgrave Square, London, S.W.1, price 2*s*.

Gulbenkian Foundation Reports

by G. R. NOAKES
Uppingham School

In April 1960 the Oxford University Department of Education reported to the Gulbenkian Foundation on *Arts and Science Sides in the Sixth Form.* In September 1959 the University of Birmingham published a similarly sponsored Report on *An Enquiry into the Suitability of the General Certificate of Education Advanced Level Syllabuses in Science as a Preparation for Direct Entry into First Degree Courses in the Faculty of Science.* As a background to these Reports many readers will have studied the draft recommendations for new O-level and A-level syllabuses in Physics which have been prepared by the Science Masters' Association and circulated to members, though they cannot at this stage be discussed publicly. On the general issue of specialization, the Crowther Report opined that this met the ' subject-minded ' intellectual needs of the sixth-form pupil, though he might outgrow this stage and develop more widely after 18, and that the usual ' three A-levels ' were, for the scientist at any rate, reasonably satisfactory. To this resonant fusillade of overt reports can be added an underground test; for although no firm information of any kind has been released, it is widely believed that some basic modifications of the general structure of the A-level examination are being planned for the near future.

The Oxford report

The main conclusion of the Oxford Report is that specialization should not of itself involve a dispersion into ' sides ', and that a wide choice of subjects for specialist study should be possible for pupils and, within certain limits, acceptable for university entrance. Once the value of intensive study is accepted, the desirability of these conditions is unquestioned; the value of this Report is that it shows that students would also prefer them, and that time-tables would in certain circumstances appear to be amenable to the simple calculations involved.

First, is the notion of ' sides ' in itself an accidental product of tradition, and of the thesis that (in the words of Edward Thring) a full education could be given through the medium of ' one noble subject '? The Report suggests that ' sides ' arose as other subjects besides the Classics claimed patents of nobility; but I do not think that this is historically correct, or that they got them anyhow. If we regard ' sides ' as the tap-roots of university faculties we shall be nearer the mark.

What special features can any given subject contribute to a pupil's development? Literature cultivates moral judgment and knowledge of men; mathematics and languages the power of logical thinking ; natural science the experimental outlook; and literature again the aesthetic faculty. All four are necessary parts of a complete education. The Report advocates four subjects, chosen from the arts and the sciences, to be studied at specialist sixth-form level, with no ' side ' bias so that, for example, Latin, Greek, Physics, and Mathematics might be taken together.

Of the objections foreseen, the first, that this would go against the natural interests of the pupil, would seem to be discounted by the answers received to a questionnaire on preferences. That it would raise administrative, staffing, and time-table difficulties within the schools is faced, and shown not to be insuperable. The question of individual capacity is another matter; the Report points out that a spread of work over six or seven O-level subjects can be managed at 16, and so the capacity for further progress is assumed. But experience suggests that there are many pupils at present who can attend properly only to two subjects at Advanced level, which is all that the old Higher Certificate required. If we are being asked to accept a lower level for four selected subjects in order to secure equality of standard amongst them, will sixth-form teaching ultimately benefit? Before considering this question, it would be well to see what the Birmingham Report, which has this in mind, has proposed.

The time allotment for a specialist subject in the Oxford four-subject scheme is seven periods per week. This is not appreciably less than the average for the present three-subject A-level course. An heroic gesture, in the form of a secession group of schools dedicated to this programme, who would arrange for a new examination drill, is cheerfully mentioned as a desperate means of breaking the present apparently self-perpetuating scheme. It is assumed that they would have to persuade universities to agree to a wider spread of A-level subjects for matriculation, and that they would automatically forego competition for Oxford and Cambridge scholarships. This is all very hearty stuff, but is it necessary? It almost seems that, enslaved by the magic of $4 \times 7 = 28$, the Report has overlooked $2 + 1 = 3$; for a high proportion of university-destined pupils spend three years in the sixth form.

The Birmingham report

The section of the Report discussed here is solely that of the Physics panel, which dealt very largely with syllabuses, question papers, and the division of material between the A-level and the S-level syllabuses, on the assumption that the latter would generally involve a third year of work. As Oxford found, the balance of subjects is distorted more heavily towards specialist subjects than the school time-table shows, because of the time spent on homework in them.

The proposals made are, first, a lightening of the A-level Physics syllabus so as to release about 10–15 per cent of the present time for other purposes, which seems to mean one period a week. Extrapolated to other specialist subjects, this yields three periods ‘ made ’. Here it would seem to be possible to insert the fourth arts subject. The syllabus, that of the Northern Universities' Joint Matriculation Board, which had previously been common for both A- and S-level is now split in much the same way as that of the Oxford and Cambridge Joint Board, with what seems a reasonable third year's work of the more difficult topics extracted for the separate S-level.

Next, a ‘ New Level ’ Physics syllabus, to be introduced as soon as practicable, intended primarily for students who do not intend to specialize in one of the physical sciences but who would benefit from a two-year cultural course or science in the sixth form. Since this includes matters like the solid state, microwaves, the transistor, and the basic elements of an electronic computer, it is going to take some mounting—though it might be possible to obtain a pass on the specimen paper given by knowing the laws of motion, the behaviour of a plane mirror, and the nature of electromagnetic induction. Examiners will have to live up to it!

DISCUSSION

We might first take the point made by Birmingham, ' that for Physics at least, the combination of a school-leaving examination with a foundation for further studies is a difficult matter '—hence the ' New Level ' paper for the former. Oxford assumes that all pupils in the sixth form can with equal profit follow the same two-year course, or at least solves the time-table problem to this effect. Now, at present, in schools that are large enough to allow it, A-level Physics candidates are separated into two groups for teaching, according to whether they intend to proceed further with the subject or not. The Birmingham point seems in keeping with experience. Is Physics the only awkward fit? Is it possible to devise a common syllabus that would fit in with the Oxford scheme, and also be acceptable to all university departments of Physics? This is where, when the S.M.A. suggestions are published, a really fruitful break-through might materialize.

The second point is this matter of the third year. The revised A-level syllabus will continue to be acceptable for entrance requirements that demand three A-levels. And Birmingham suggests that when the ' New Level ' spreads to arts subjects, a suitable combination for entrance might be two A-level subjects plus two ' New Level ' subjects. (It is here implied that *all* subjects can well be taught in two different ways in the sixth form ; and this is a basic difference in view between the two Reports.) Birmingham says that, though the university Physics faculty will admit on the two-year A-level, they would like people to develop new interests in the third sixth-form year and take the S-level course. Here we might hope for some reconsideration of the purpose of this third year. Is it to continue to be directed towards competition for State scholarships, which merely shift support from the rates to the taxes, or for open Oxford and Cambridge awards, of which it can only be said that the kind of ability that they are intended to recognize is usually manifest a good deal earlier in life? It would seem important to allow the pupil who has chosen his future study and been accepted for it to specialize as fully, or to prepare himself for further education as generally, as his capacity allows. And it is this higher work that provides a challenge and incentive to the teacher also, irrespective of the competitive basis on which it is at present assessed. A third Report, on the pre-university school year, might be useful in tying up the loose threads.

Science Education in a Scientific Age

This was the title of the address with which Lord Hailsham, Lord Privy Seal and Minister for Science, opened the O.E.E.C. Seminar on Policy for School Science at Brussels on April 28th, 1960.

Posing the general question ' Why are we teaching natural science ? ', the Minister pointed to the value of scientific discipline for training the critical faculty, and introducing young people to basic truths which—while not the monopoly of science—can be learned from it as from other disciplines. He spoke of the great potential future development of science, and suggested that it should be taught as a young study capable of almost indefinite expansion, and not as a corpus of final and unalterable truths. The Minister also referred to the interdependence of scientific and humane studies in the education of the fully-developed individual, and to the satisfaction which can be gained from learning of the wonders of the physical world.

Pointing out that the seminar was intended for professional scientists and educationalists, the Minister referred to the wide-spread view among such persons that there is a need for pruning the science teaching curricula in schools of much tedious and obsolete material. He welcomed the contribution which O.E.E.C. hopes to make to the improvement of science curricula through seminars and meetings, such as the present one. The Minister, speaking not as a scientist or educationalist, but as a politician, touched on the following points as possible subjects for discussion :

(1) The aims of scientific education :
   (*a*) for democracy as a whole ;
   (*b*) for those who have to take decisions ;
   (*c*) for the scientific aristocracy and, as a corollary, the incompleteness of science and the need for complementary humane studies.
(2) The place of mathematics in science teaching.
(3) The scope for scientific training at all stages of education, i.e., even for young children.
(4) The balance that has to be struck in school teaching, between specialization and general education.
(5) The education of technicians.
(6) The scientific training of girls.
(7) The shortage of science teachers.

The Minister emphasized the last point, ' instancing some of its serious manifestations '. And no joyful ones. It is hard to reconcile oneself to the view that all that those in charge of our affairs can do about it is to exchange wise saws and modern instances, and point to the legacy of the past. But one can hope that the Minister has some plans about it for the future.

ESSAY REVIEW

Advances in Spectroscopy, Vol. I. Edited by H. W. Thompson. (London and New York : Interscience Publishers Ltd., 1959.) [Pp. ix + 363.] £4. 4*s*.

Spectroscopy studies electromagnetic radiation associated with energy transitions in quantized systems. Its experimental findings constitute a challenge to the theoretical physicist to devise a consistent mathematical ' model ' capable of generating the energy levels involved and also the selection rules limiting the possibility of transitions between them.

Not until Bohr's treatment of the hydrogen atom (1913) was any theoretical answer forthcoming. A great deal of empirically ordered spectroscopic evidence was available at that time. Much of it had been obtained in the visible region, some also from the neighbouring ultra-violet and infra-red. In the last few decades quantum theory has been greatly developed, and this has been largely due to the remarkable extension of the scope of spectroscopy.

The wide range of frequencies now studied corresponds to the wide range of forces operative in different quantized systems. At the one extreme are γ-rays, concerned with energy levels of atomic nuclei. Here the frequencies are of the order of 10^{21} cycles per second. At the other extreme are radio waves with frequencies of the order of 10^6 cycles per second, i.e. smaller by the enormous factor 10^{15}. These are concerned with transitions of nuclei between different spin states in an applied magnetic field. Between the extremes come successively the X-ray, vacuum ultra-violet, ultra-violet and visible regions (concerned with transitions of electrons in the field of nuclei), followed by the infra-red and micro-wave regions (concerned with molecular vibrations and rotations and with transitions of electrons between spin states in an applied magnetic field).

Nowadays to designate someone as a spectroscopist is to give but a very imprecise idea of the sort of work he may be doing: rather like designating someone as a musician without specifying whether he plays the piccolo or the double-bass. So great are the differences, both in experimental methods and in theory, between the different branches of spectroscopy that no individual can be master of all of them. But equally no specialist can afford to remain ignorant of results obtained by techniques different from his own. Clearly therefore (to quote the Introduction to the volume under review) " there is a need for periodic authoritative surveys on recent progress . . . collected together in one place ". This book appears as Volume I of a projected series, designed to satisfy this need. It includes accounts of special topics concerned with the ultra-violet, visible and infra-red regions. Subsequent volumes will deal with other topics and fields. All written by specialists about their own particular lines of work, the articles are primarily intended for the information of other specialists, and full appreciation presupposes considerable spectroscopic knowledge. Nevertheless the general reader will find much to interest him, for the contributions are very readable and mainly non-mathematical. Considerable space is devoted to reviews of recent results, and these will be interesting even to readers who are unable to understand fully how they have been deduced from the experimental observations.

Two articles, both by D. H. Rank (Pennsylvania State University, U.S.A.), differ from the rest in that they are comparatively short (3 and 12 pages respectively) and deal, not with spectroscopy directly, but with measurements of two quantities (the refractive index of air, μ_{air}, and the velocity of light *in vacuo*, *c*), the exact values of which are of importance for the conversion of wavelengths, measured in air, to frequencies and hence to the energies which are the ultimately interesting quantities. Rank concludes that recent determinations of μ_{air} confirm the validity of the Edlén dispersion formula over the whole range of ultra-violet, visible and infra-red. His second article is an interesting review of the experimental methods now available for the determination of *c*, including the use of microwaves of accurately known frequency. Results are critically assessed and the weighted value $c = 299\,793{\cdot}04 \pm 0{\cdot}17$ km per sec. is derived. This is about 16 to 17 km per sec. higher than the mean of values determined before 1948. As for all the articles in the book, an extensive list of references is given.

W. C. Price (King's College, London) contributes a general account (20 pages) of spectroscopy in the vacuum ultra-violet, i.e. in the range from about 1850 Å down to roughly 100 Å, which is absorbed by air. Experimental techniques are first described: sources of continuous radiation, especially rare-gas continua; grating spectrographs and monochromators; and detectors, including photomultipliers and photosensitive Geiger counters. A review of results is then given. These include absorption by atoms due to excitation of ' inner ' electrons, and it is noted that commercial vacuum ultra-violet equipment is now available for the spectrochemical analysis of C, S, P, As, Se and all metallic elements. Electronic transitions in molecules are accompanied by simultaneous vibrational and rotational transitions, and the structures of the resulting bands have been analysed with high-resolution instruments in the case of some diatomic species. Polyatomic molecules may show Rydberg series, from which information can be derived about the energies and configurations of the excited states of the molecules and molecular ions. The review also deals with the determination of accurate ionisation potentials of complex polyatomic molecules by the use of vacuum monochromators with attached ionisation chambers.

Some free radicals (e.g. triphenyl methyl) are quite stable, despite their odd electron which gives them the character of molecular fragments rather than normal molecules. But many free radicals produced by the disruption of stable molecules are extremely short-lived, and the investigation of their structure requires very specialized spectroscopic techniques, mainly in the ultra-violet. D. A. Ramsay (National Research Council Laboratory, Ottawa) contributes an article (55 pages) on the spectra of polyatomic free radicals of this kind. He reviews experimental techniques, including flash photolysis in which high concentrations of radicals are produced momentarily by the photo-disruption of molecules by an intense flash, and immediately afterwards their absorption spectra are photographed by the use of a suitable second flash. More than half the article is devoted to results, principally from rotational analyses. The reader not possessing a good knowledge of the quantum theory of molecules and the significance of the symbols used will probably have to content himself with an examination of the table of bond-lengths and bond-angles, etc. He may well marvel that such precise determinations can be made for such ephemeral molecular fragments.

Infra-red spectroscopy is the main subject of two articles. One by T. S. Moss (Royal Aircraft Establishment, Farnborough) deals exclusively with infra-red detectors (39 pages) and is perhaps too specialized for the average reader. Detectors are of two types: thermal detectors in which the incident radiation warms the receiving element and produces some observable change in its physical properties, and quantum detectors in which individual photons excite electrons and so give rise to measurable phenomena. The former type includes thermocouples, bolometers and pneumatic (Golay) cells: the latter includes photoconductive devices. The theory of the various detectors is discussed and their practical performances critically assessed.

The article by A. Elliott (Courtauld Research Laboratory, Maidenhead) on the infra-red spectra of polymers may be divided into three parts. The first (20 pages) reviews the appropriate special techniques, notably infra-red microscopy for the examination of small samples and fibres and the use of polarized infra-red radiation for the investigation of oriented samples. The second part (32 pages) describes some of the results obtained. It begins with a brief outline of the theory of vibrations in long chains (exemplified by polyethylene whose spectrum " is to the student of polymer spectra what the spectrum of H_2 is to those working with diatomic molecules "), and goes on to report findings for special systems such as cellulose, synthetic polypeptides and proteins. The author concludes that " in view of the complexity of polymer spectra . . . it is likely to be a long time before complete understanding of the spectra of any but the simplest polymers is reached ". The third part is a list of references. Doubtless of great value to the specialist, its extent (21 pages) is extravagant for the general reader.

In the rotation of one part of a molecule with respect to another about a chemical bond (e.g. the rotation of one of the methyl groups of ethane with respect to the other about the C—C bond) the potential energy passes through minima which, if the maxima between them are sufficiently high, may correspond to different stable configurations (called rotational isomers) and so give rise to characteristic vibrational frequencies in infra-red and Raman spectra. By studying these spectra information can be obtained about the shapes of the isomers and the energy differences between them. Much of the interest of this work lies in the fact that the reason for the potential minima is not fully understood. Results on rotation about C—C bonds in saturated molecules are reviewed (65 pages) by N. Sheppard (Cambridge University). After a brief account of general principles, there are sections on substituted ethanes, polymethylene chains (overlapping to some extent with Elliott's article on polymers) and cyclic compounds such as substituted *cyclo*-hexanes. This article will be most appreciated by readers with some previous knowledge of molecular vibrations.

Raman spectra, unlike emission and absorption spectra, are produced when visible light is scattered by molecules. The collision of a photon (energy $h\nu_0$) may induce a molecule to make a vibrational or rotational transition (energy $h\nu$) either to a higher or a lower level. The energy requisite for an upward transition is taken from the photon, which is thereby impoverished and scattered with the diminished frequency ($\nu_0 - \nu$). A downward transition gives energy to the photon, which is thereby enriched and scattered with the enhanced frequency ($\nu_0 + \nu$). Thus vibrational and rotational frequencies (such as are encountered in emission and absorption in the infra-red and micro-wave regions) appear in the visible as frequency shifts ; but since the mechanism of the Raman effect is quite different from that of emission and absorption, different selection rules operate. Raman spectra are very feeble and by far the most work has been done on vibrational frequencies with liquid samples, where the shifts can be conveniently observed with luminous spectrographs of comparatively low resolving power. Observation of pure rotational Raman shifts requires the use of gaseous samples at pressures of one atmosphere or less, and high-resolution instruments are essential. The article by B. Stoicheff (National Research Council Laboratory, Ottawa) on this special branch of Raman spectroscopy is the longest (84 pages) in the volume. It is true that precise determinations of rotational constants and hence of molecular parameters can be made by micro-wave spectroscopy; but only for molecules possessing an electric dipole moment. The importance of the Raman method is that it is applicable to molecules with no dipole moment. Stoicheff, using the Ottawa 21-foot grating, has himself been responsible for most of the recent advances, and his article is a welcome précis of his own important work over the last few years. It includes a résumé of the theory of Raman scattering and a description of the techniques of high-resolution work (intense light sources, multi-reflection sample tubes, etc.). Elegant pure rotational spectrograms are reproduced. The account of the results necessarily pre-supposes a familiarity with spectroscopic theory and nomenclature, and its difficulty increases as first linear molecules, then symmetrical tops and finally asymmetrical tops are discussed.

Layout and printing of text and diagrams are excellent throughout. The contributors are all distinguished authorities who write clearly and as simply as their subjects allow. If subsequent volumes maintain the same high standard, the series will surely succeed in its objective.

L. A. WOODWARD.

BOOK REVIEWS

A Philosopher Looks at Science. By JOHN G. KEMENY. (Van Nostrand, 1959.) [Pp. xii + 273.] 37*s.* 6*d.*

This book is of a type which does not readily find readers in this country; it is essentially attuned to the 'climate' of the United States. But that it leaves little to be desired in the way of sincerity and clarity of expression is obvious. The author (a philosopher) does what he says: he looks at science. And this he achieves by dividing his theme into three main sections, namely, I, What science presupposes, II, Science, III, Problems raised by science. Fundamental as probability-theory is for all this, it becomes perhaps a trifle laboured in the writer's hands. He assumes quite rightly that his readers are laymen in these matters; nevertheless, even the fringe of the subject is not easy, and it might therefore have been better to clip it considerably, or shorten it by making it more mathematically formal. On the other hand, the sketch of non-Euclidean geometries could not be bettered, and the upshot of the observations on teleology—although the author does not call it so explicitly in this context—that science provides means rather than ends, is admirable.

With us at least, these pages should provide a stimulating springboard for a sixth-form seminar, as well as being of interest to a wider public. F. I. G. RAWLINS.

Principles of General Physiology. Vol. I: The Physico-Chemical Background. By L. E. BAYLISS. (Longmans, 1959.) [Pp. xiii + 520] 55*s.*

Dr. Bayliss has re-written his father's much admired book, which was originally published in 1915 and had received only partial revision since, and has collected into one volume all, or nearly all, the physics and chemistry that a physiologist needs to know.

I have tested the book both on subjects that I think I understand and on those that I know I do not, and while I have sometimes been helped on the latter, I have generally felt that the former could have been put more clearly. The exposition of the kinetic theory of gases, for example, omits the essential point that the molecules must be perfectly elastic, and, in order to avoid the difficulty of the resolution of velocity, supposes them to be moving in a way which, if it were possible, would lead to the bursting of any vessel that contained them. On enzymes Dr. Bayliss errs in good company, but we are told on page 295 that an enzyme is a catalyst and that a catalyst is not a reactant, and on page 310 that the enzyme forms a compound with its substrate. While the critical teacher will find this book useful on his own shelves, he will, I think, hesitate to recommend it to students until it has been polished for another edition; this is a pity, because it has clearly involved much hard work and is a worthy attempt at a difficult task. W. B. YAPP.

Dynamics. By A. E. SHORT. (London: University of London Press, Ltd., 1959.) [Pp. 384.] 30*s.*

This book is intended for sixth forms and for the first year of university courses. It goes well beyond General Certificate of Education Advanced Level standard, and includes such topics as motion in a resisting medium, central orbits and some problems involving variable mass, as well as some two-dimensional rigid dynamics. The author suggests that the book " makes full use of vector analysis ", but this is only true in the sense that there is an early chapter on vector addition, scalar and vector products, and differentiation of vectors with respect to a scalar quantity, and that a few vectors are scattered throughout the rest of the book. The treatment is almost entirely cartesian, but this is lucid and well illustrated by examples. There are also many sets of problems for the reader, but again, except in the chapter devoted to vectors, there are few demanding a vector solution.

The term kineton is used for the rate of change of momentum, so that, for example, d'Alembert's Principle is expressed in the form : " the kineta of the particles which constitute a rigid body, when it is in motion, are together equivalent to the system of external forces acting upon it ". In particle problems the forces and the kineta are represented in one figure, but with rigid bodies two diagrams are usually drawn in one of which the forces are represented and in the other the kineta, and the conditions of equivalence are applied; for some students this will be a help towards finding the correct mathematical solution, but one wonders what it contributes to a physical understanding of the problem.

J. TOPPING.

The Human Element in Research Management. By B. E. Noltingk. (Elsevier, 1959.) [Pp. 92.] 9*s.* 6*d.*

This is an interesting, stimulating, and often entertaining book. Its stressing of the human relationships set up in the making of a research team from a group of essentially individual characters has implications beyond the organization of a laboratory. The precepts set out could be applied to the type of engineering experimental departments within a large industrial organization, which the author probably considers to exist at the very boundary of his thesis. Then, of course, the chapters on the anatomy of the laboratory, and strategy of attack on problems are not altogether relevant. One cannot move engineering plant around so easily as most chemical or physical apparatus. And while it may be desirable, as he says, that the scientist should have the use of a lathe if he wants it, a research engineer in the workshop might have surprising repercussions. Has Dr. Noltingk not heard of trade unions ? And the idea that a valuable scientist with no administrative responsibility might be paid more than his (nominal) superior is surely tantamount to heresy in our materialist society. Within its slim compass, however, this volume includes a great deal more common sense than many more pretentious works. It is essential reading for the junior entering the world of scientific or technological research and should be brought tactfully to the notice of every director of research. But is one right in thinking that the author has little love for the accountant-administrator ? J. Spiers.

Annual Report of the Advisory Council on Scientific Policy 1958–59. (Cmnd. 893, H.M.S.O., November 1959.) 1*s.*

This Report, in six sections, deals with

I. *Scientific man-power:* In 1958, 5700 scientists and 8900 engineers were trained. It is unlikely that the universities and C.A.T.s are training more scientists than are needed, except in biology. It is suggested that the biologist of average attainment should consider acquiring supporting qualifications either in mathematics or one of the physical sciences for teaching purposes. Teachers are advised to impress this position on their pupils; and clearly, since it is in the nature of biologists to drop mathematics in the sixth form, it is a matter to be reckoned with at an early stage.

II. *Space research:* A joint programme with the United States (see the article in our second issue by Sir Harrie Massey), in which British instruments will be accommodated in American earth satellites during the next three or four years, and participation in the joint United States–Canadian ionosphere programme, have been agreed. Design studies for the future adaptation of British military rockets are in hand. The annual cost of the programme is estimated as £100 000–£200 000.

III. *National Lending Library of science and technology:* The D.S.I.R. plans this to be in full operation by 1962, established at Thorp Arch in Yorkshire; the lending library unit is collecting literature, is sponsoring complete translations of eight Russian periodicals, and hopes to add to this number.

IV. *United States' contacts:* Financial support to the extent of £1¼ millions per annum from United States Government and charitable foundations is going to research in this country.

V. *Overseas scientific relations:* The N.A.T.O. Science Committee is supporting post-graduate fellowships, short summer courses at advanced study institutes, and grants for promoting co-operation in research; a sub-committee for oceanographic research has been set up, and a study group to review the general progress of scientific effort in the N.A.T.O. countries. The Organization for European Economic Co-operation (O.E.E.C.) has organized symposia on the teaching of science and mathematics in schools, and maintained its Senior Visiting Fellowship programme. An Overseas Research Council, under the chairmanship of Dr. R. S. Aitken, Vice-Chancellor of Birmingham University, was established to co-ordinate developments within the Commonwealth, particularly in the new territories moving towards independence.

VI. *Statistics:* In these figures prominence is given to the number of graduate teachers of science and mathematics in the schools, a total of 14 246, which rose by 315 in 1957-58. This works out to about 5 per cent of the total teaching force, and averages half a graduate per secondary modern school.

Farbenphotographie und Farbenfilm. By DR. WERNER SCHULTZE. (Berlin: Springer-Verlag, 1953.) [Pp. vii + 318.] DM 40.

One of the most surprising things in the history of colour-photography is that the most complex and improbable-sounding processes are the only ones to have survived commercially, whereas the method that looked so simple when Clerk Maxwell demonstrated it just a century ago has defied all the attempts of optical engineers to make it practicable. In this book Dr. Schultze covers a great deal of ground, starting with a short historical survey in which he tries to put the various methods into perspective as he proceeds. A section on the basic principles of colour-photography follows, with a broad classification of the different methods, and detailed descriptions of the best-known processes in both ciné and still photography, in which a uniform type of coloured diagram greatly facilitates comparison.

The second half of the book deals with colorimetry and colour systems, theory of colour reproduction, sensitometry and masking, and has a final section on apparatus, materials and procedures. Since some subjects are touched upon at more than one level, there is bound to be a certain amount of overlapping. The text is very fully documented throughout, and all references are to a bibliography at the end, which being partly classified is useful in itself. Actual specimens of colour-film and a colour enlargement are included.

M. H. WILSON.

Extensive Air Showers. By W. GALBRAITH. (London: Butterworths, 1958.) [Pp. xvi + 211.] 40*s*.

Extensive showers have now been known for rather more than twenty years. For almost as long, it has been recognized that most of the particles in them are electrons, and that these form the directly observable part of electron–photon cascades; the theory of such cascades has been developed with confidence and in great detail, and appears to be valid to the highest energies. A source of electrons or photons from which such cascades develop is less obvious (since the geomagnetic deflection of the primary particles reaching the earth indicates that these are positively charged), and it is only in the last ten years that the essential step, in which uncharged mesons, formed in energetic nuclear collisions, undergo free decay into photons, has been understood. The nuclear collisions are those of primary cosmic ray particles, and of secondary nucleons arising in successive encounters from them, with the nuclei of the atmospheric gases. Details of these collisions at the great energies involved in showers, say from 10^{12} ev up at least to 10^{17} ev, including such features as the multiplicity, energy spectrum and angular distribution of emitted mesons, are of extreme interest, as is almost always the extension of study of any physical phenomenon to new orders of magnitude in energy. Study of these collisions is one objective of current work on extensive showers: as important are the conclusions to be drawn, through an understanding of the atmospheric processes as a whole, about the energy distribution of the primary particles which are responsible for showers.

Dr. Galbraith is mainly concerned to survey and to order coherently the numerous and varied experiments which have been reported from all over the world in recent years. The variety of these, which is immediately notable, arises from the differing characteristics of available detecting devices, from the fact that all shower studies involve sampling over more or less of the great area covered by each shower, from the necessity of studies in depth through the atmosphere and the limitations of design imposed in non-earthbound equipment, and probably most of all from the number of alternative features of the shower complex which can be chosen even at a single observing point. It makes a work such as the present urgently needed.

The survey is admirable indeed for those most likely to use it, that is, people working in the subject or planning to do so; probably for the first time it will be reasonably straightforward to survey the greater part of significant work over the past twenty years. It is no criticism of a most valuable book to suggest that it will not be of comparable interest for most other readers. It is in essence a progress report at a stage when, while prime objectives can be recognised, only the slightest conclusions can yet be drawn about them. These are the features which, as they come to be understood, will be of wide interest and importance, in fields as far separated as fundamental particle theory and cosmology. What is now available, however, is much more restricted, and its interest, therefore, of necessity much more specialized.

J. G. WILSON.

The Chemistry of Organic Compounds. By J. B. CONANT and A. H. BLATT. 5th Edition. (New York: The Macmillan Co., 1959.) [Pp. viii + 652.] 54*s*.

This is the fifth edition of a text-book which first appeared in 1933. The sub-title describes it as a year's course, and from the text it is clearly intended to be a first-year book and therefore an introduction to the subject. In earlier editions the subject was introduced by way of the alcohols and it is difficult to understand why this method was changed.

The first chapter now contains a very brief discussion on structure and valency. Here, surprisingly, the concept of atomic orbitals is briefly treated as a revision topic. The spotty diagrams of these orbitals can be forgiven, but the representation of ethyleny on page 31 is incorrect.

This is followed by the usual systematic studies beginning with the paraffin hydrocarbons. It is a pity that the conventional strict division of aliphatic and aromatic compounds is retained. As might be expected, American industrial methods are given prominence.

The treatment on the whole is rather inclined to be superficial and this, together with colloquial introductions, makes the text easy to read. Search for some mention of any modern topic was usually successful. The last few chapters are good; if they were intended to give a glimpse of modern organic chemistry and whet the appetite, they serve their purpose admirably. W. SIDDALL.

Physics and Electronics in Physical Medicine. By A. NIGHTINGALE. (London : G. Bell & Sons, 1959.) [Pp. x + 292.] 30*s*.

Dr. Alfred Nightingale has for several years been especially connected with courses of instruction for medical graduates intending to specialize in physical medicine and for student physiotherapists. This book presents the basic physics such students must cover, together with an account of the specialized applications in physical medicine of electricity and radiations.

Part I (149 pages) deals with the fundamentals, and within the limitation set by this number of pages the author has done remarkably well. There is much compression and omission in the chapters on mechanics and heat, but electricity, electronics and radiation are dealt with more adequately in the following chapters. Part II (69 pages) gives an elementary and concise account of the electrical and electronic generators used in physical medicine, of diathermy, and of the biological effects and medical applications of infra-red and ultra-violet radiation. Some more advanced topics (e.g. electromyography, ultrasonics, transistors) are covered in eight appendices comprising Part III.

This is a textbook of great value to students of physiotherapy and of physical medicine. It is written lucidly, the diagrams are excellent, and there are very few errors or misprints (the confusion of Wien's with Stefan's law on p. 50 is the most obvious error). Those who teach, or need to study, this particular branch of medical physics at an elementary level will find much of interest to them in this book. H. F. COOK.

Nuclear Reactor Materials. By B. R. T. FROST and M. B. WALDRON. (London: Temple Press, 1959.) [Pp. vii + 79.] 12*s*. 6*d*.

It is a common impression amongst physicists, engineers, and metallurgists engaged on the design of nuclear reactors that their own subject forms the basis of the design. In fact, the success of the project depends very greatly upon the extent to which each understands the others' problems. A monograph which states, in terms intelligible to the engineer and physicist, the particular difficulties facing the metallurgist is therefore of great potential value.

Two main sources of difficulty stem firstly from the limitation placed on the choice of reactor materials by neutron absorption, and secondly from the effect of a neutron flux on the physical and mechanical properties of the material. The severity of the damage that may be inflicted upon a metal in the core of a reactor can be judged from the fact that, in a lifetime of about twenty years, every atom is knocked out of its place in the crystal lattice at least once. The damage in the fuel is on an even larger scale, and may fairly be said to constitute the critical problem in the development of economic nuclear power.

The authors of the monograph were clearly faced with the choice between at least two alternative courses; they could either present a conventional metallurgical description of those metals which the restriction of neutron absorption permits them to use, or they could restrict themselves to the solid state physics problems of neutron interaction with matter. That they have leaned towards the first alternative may make the monograph of greater interest to engineers than to physicists. The description of specific reactor materials is prefaced by a summary of the more important aspects of modern metallurgical theory which, although more condensed than one could have wished, is nevertheless clear and well planned. On balance the result justifies the authors' choice of treatment, and should provide reactor designers with a sound introduction to reactor materials problems, and with a stimulus to take a deeper interest in the physical processes involved. W. B. HALL.

Techniques Modernes et Applications de la Cryométrie. By Y. DOUCET. (Paris : Dunod, 1959.) [Pp. 225.]

Cryometry is concerned with the change in the freezing-point of a pure substance (or in the eutectic point or transition point of a salt hydrate) on the addition of a second substance. This book describes the experimental techniques involved and discusses the applications of cryoscopic measurements. Professor Doucet has performed a useful service in writing it, since cryometry, so far from becoming out-of-date, will always have its place in experimental chemistry, and, in fact, is likely to be applied increasingly to solutions in which the solvent is a molten salt or an ionizing liquid other than water.

The book is clearly written and well illustrated, and it contains much useful information not only about the measurement of freezing-points but also about thermometers, thermostats, the realization of thermometric fixed points, and the analysis of solutions by physical methods. The value and potentialities of cryometry, however, might perhaps have been more strikingly demonstrated. Thus, there is no mention of the important work of Gillespie and of Giauque using sulphuric acid as solvent, or of the Rast camphor technique used by the organic chemist for molecular weight determinations, while the account of the evaluation of excess thermodynamic functions from cryoscopic measurements (a method which may find increasing application to binary systems of non-electrolytes) would have been improved if it had been illustrated by an actual example. Finally, in the brief account of the use of freezing-point measurements to determine the purity of a substance, there is no mention of what is probably the most thorough and elegant investigation of this kind yet undertaken, namely that of Glasgow and his colleagues at the National Bureau of Standards on various samples of benzene. L. A. K. STAVELEY.

The General Properties of Matter. A 48-page handbook published and issued with the Home Scientist sets marketed by Kay (Sports and Games) Ltd., The Hyde, London, N.W.7.

A chance enquiry about the availability of physical apparatus in the instructional-toy market brought me a complete kit from the suppliers, with this booklet which certainly merits notice. It describes simple experiments on the spring balance, the pendulum, friction, pressure, surface tension, thermal expansion, conduction, and convection. It is sensibly written and on the right lines generally. The outfit as a whole seems to try to go too far in some directions and to miss opportunities in others; it is also (at £3. 9*s*. 6*d*.) expensive for what it contains. But it is something that one welcomes and would like to see developed to its full possibilities. G. R. NOAKES.

The Prof: A Personal Memoir of Lord Cherwell. By R. F. HARROD. (London: Macmillan & Co., 1959.) [Pp. xvi+282.] 25*s*.

The subtitle, a personal memoir, fairly describes this account of Sir Roy Harrod's friendship as a colleague at Christ Church and in the S branch with the late Lord Cherwell, lasting over some thirty years. For that is what it is, and as the author would agree it sheds a great deal of light on the personalities of both, not omitting what appear to be some unsympathetic touches in each case. A full appreciation of The Prof.'s contribution to science, and his work in establishing the Clarendon Laboratory as we know it today, has been given by Sir George Thomson in *Biographical Memoirs of Fellows of the Royal Society*, vol. 4, November 1958; some people may have regretted that this book is not such an objective tribute to his achievement. But it does not profess to be.

There were still Oxford Characters in the 1920's, and The Prof. was certainly one of them. Was he really a more difficult lecturer than Townsend, as unproductive as Soddy, or anywhere near Cruttwell when inspired to unkind repartee? The public figure presented to the university was highly regarded; the domestic politics in which he engaged were, one imagines, an inescapable part of general academic life at least for a scientist. The first part of the book, on the inter-war years is, in effect, a criticism of those times; the second part describes the part he played behind the scenes and subsequently more publicly in the war and its aftermath, as seen through the author's eyes.

Introductory Nuclear Theory. By DR. L. R. B. ELTON. (London : Pitman, 1959.) [Pp. xi + 286.] 40*s*.

The present incomplete knowledge of the nature of the forces between nucleons makes impossible a logical theoretical deduction of the properties of nuclei. Dr. Elton has chosen therefore the phenomenological approach; starting with a qualitative description of the nucleus and its constituents, he gives a quantum-mechanical account of the two nucleon system at low energies covering the important scattering processes for n–p and p–p systems, referring at all times to experimental results. This is followed by chapters on Nuclear Forces, Nuclear Reactions and Nuclear Disintegrations including an excellent summary of the physical models used in predicting the properties of nuclei. The time-dependent perturbation theory is introduced and used to account for the interaction of nuclei with the electromagnetic field. In the final two chapters relativistic quantum-mechanics has to be invoked to discuss β-decay and to give a short treatment of the meson theory of the nucleus.

The book is based on a lecture course given to final honours physics students at King's College, London, and a selection of examples (together with hints for solutions) is given at the end of each chapter. Of necessity in a book of this size there is considerable compression of complex mathematical arguments and the lone student who is not able to call upon a Dr. Elton to resolve his difficulties is advised to provide himself with a reference work on quantum mechanics.

The book is thoroughly up-to-date (there is a section on the non-conservation of parity), and stands comparison with the classic Blatt and Weisskopf's *Theoretical Nuclear Physics*. It can be whole-heartedly recommended to all theoretical nuclear physicists.

W. K. MANSFIELD.

Les Satellites Artificiels et l'Astronautique. By R. ANDRÉ. (Paris: Dunod, 1959.) [Pp. 126.]

This very readable little book gives a clear and concise account of the problems involved in the projection of artificial satellites, and of the possible methods of overcoming the difficulties. It starts with a discussion of types of trajectories. From this it proceeds to projection into a predetermined orbit, the energy required, and the means of producing this energy by a multi-stage rocket. The possibility of reaching Venus and Mars is considered and also the practical impossibility of a manned projectile exploring outer space. All this is most interestingly and logically developed, and should make good reading for scientist and non-scientist alike. Recourse to a dictionary should hardly be necessary. The appendices give the elementary theory involved; this should already be familiar to any sixth-form mathematician or scientist, but it completes a most engaging and up-to-date account of space-travel.

D. S. DUNBAR.

The Elements of Determinants, Matrices and Tensors for Engineers. By A. AUSTEN STIGANT. (London: Macdonald, 1959.) [Pp. xi + 433.] 60*s*.

A main aim of the author is to present in an elementary way the fundamentals of determinants, matrices and tensors to electrical engineers. The final part of the book is concerned with the application of tensor methods to stationary electrical networks, and should serve as a useful introduction to the work and writings of Kron and others.

Readers with good mathematical ability will find the treatment of determinants much too slow and laboured. Others may find the excessive detail misleading, in that the methods will seem cumbersome and unattractive. One appreciates the author's technique of leading to general rules through particular examples, but the arithmetic is often impossible. Of less importance, the word ‘ proof ’ used in many of the examples should be replaced by ‘ check ’.

Again, in the chapters on matrices, the explanation is too detailed and the algebra forbidding. However, readers who work through all this will certainly be able to find the inverse of a matrix, to multiply matrices, and to solve algebraic equations by matrix methods. Tensors are introduced by using simple electrical networks; their properties are developed, and later tensor methods are used to solve more complex networks.

One suspects the book would have been better for being shorter, but if an engineer is looking for a competent account of the subject, and does not mind excessive detail or absence of elegance, and particularly if he has not the help of a teacher, he should find this book useful. J. TOPPING.

Physikalisch-chemisches Rechnen in Wissenschaft und Technik. By HANS FROMHERZ. (Verlag Chemie, Weinheim/Bergstrasse.) [Pp. ix + 316.] DM 32.50.

An adequate training in the application of the fundamental relationships and in the working of numerical problems is an essential part of physical chemistry. This book sets out in a clear and exact manner the basic laws of physical chemistry and the detailed solution of 206 selected problems.

The theoretical section which precedes the problems although mainly a collection of working formulae emphasises matters which the student usually finds difficult, especially in regard to definitions and units. The various sections of the book deal with kinetic theory and its applications, properties of solutions, thermochemistry, thermodynamic equilibria, electrochemistry and reaction kinetics. It is the practice throughout the book to present the problem which is then followed by the complete solution. The data have been selected from published experimental work and special attention is given to problems of practical and technical importance. No additional exercises or questions are set for the reader to work out.

The book is undoubtedly of value to students wishing to gain experience in the solution of problems and to those people in industry who need a reference to important physical relationships and their use. The production is of high quality and the contents are well arranged and illustrated. C. E. H. BAWN.

University Entrance. The Basic Facts. Published by the National Union of Teachers, 1959. [Pp. 48.] 3*s.* 6*d.*

A useful guide on university entrance requirements, the wild diversity of faculty requirements, the general procedure of application for admission, and the present grants system. It is well produced and clearly set out, and all sixth-form teachers will find it valuable.

A Career in Education: Prepared by the Central Office of Information. *Becoming a Teacher:* Ministry of Education. Both available free from the Ministry of Education, Curzon Street, London, W.1.

Shrinking almost timidly from venturing what I thought was an unprecedented suggestion, I said something in a review last time about the desirability of propaganda for a Career in Education. I did not know about these recent publications then, though they had already been whispering from the housetops for a bit. Nevertheless, I hit the right introductory note for them, for they shrink almost timidly. Seedsmen, soapmen, and poolmen produce more attractive and persuasive literature; itinerant sellers of carpets and buyers of old-false-teeth-and-jewellery put their case more forcefully in print. One can, I suppose, be thankful that these pamphlets are not abjectly apologetic, and will not deter or dissuade the young person who has already decided to teach; that is at least a step in the right direction.

The first, directed to university students, points out that teachers are needed; it goes so far as to say that the number of teachers of science and mathematics should be ‘ stepped up ', and that career prospects have been ' enhanced '. Give me my hance of burning gold! Some sort of urgent personal appeal, the accusing eyes and the pointing finger, ' David McAdam Eccles needs YOU! ' is the line that anyone who meant business would take. How can a service that demands above all enthusiasm be recruited without some display of enthusiasm at the top? There are bits later on about the ' cut and thrust of young minds ', and all the rest, that you can guess. And, of course, a list of the qualities required in a teacher that seems a gratuitous affront to common sense; loyalty, integrity, energy —the ordinary equipment of the ordinary man—should do for a start. I would recommend that the Ministry scraps the publication, substitutes something with a sense of urgency that will appeal to university students, and sees that every one of them receives a copy.

The second booklet is intended for pupils who are still at school, and is clearly aimed at a wide age-range. It starts by explaining that education is essential for everyone; that teachers deal with children (and their parents) and have many skills ; and that most schools in England are maintained, and their staffs paid. This is useful background knowledge; particularly for the pupil who has not divined what teaching means. It does continue with instructions about qualifications and entry for training colleges, and figures about salaries and pensions. On the last page is a paragraph, written with the fire and passion so lacking elsewhere, extolling the value of the work and the joys it can give. But why is this in smaller print than the rest? Here, the idea is good; and if it were carried out in an attractive style, and written in a way that would really carry conviction to the pupil of 16 or so, it would be useful.

I would suggest also two more booklets, directed to other quarters. The first to teachers, emphasizing the duty that lies on them to bring the importance and value of the teaching service before their pupils, and to encourage them to think of it. The second, to industrial firms, suggesting that part of their endowment of university-held scholarships might be earmarked for intending teachers; or that they might, while taking what are in effect long-term steps to ensure their own continuity, look just one stage further ahead in this matter—for there will be more good men available for them if there are more good teachers.

And, whatever else they publish in this line, I hope that the compilers will try to reflect on its effect on young people who have enthusiasm to be directed. One final point. Would it not be fair to give figures showing, not the salary of a deputy headmaster, but just how the average salary of a 30–40 year old graduate in teaching compares with the *average* expectation at that age in other professions and in industry? The general informed opinion seems to be that teaching doesn't come off so badly. If this is so, it should be said; if it is not so, it should be thought about pretty hard. G. R. Noakes.

Teaching in Comprehensive Schools. A first report issued by the Incorporated Association of Assistant Masters in Secondary Schools. (London: Cambridge University Press, 1960 n.p.) [Pp. 48.]

This is the kind of publication that exalts a man's pride in his profession. The I.A.A.M. were, and still are, opposed to the introduction of schemes of comprehensive education on a large scale before their value has been properly assessed, and against the disintegration of established grammar schools. Not as a matter of policy, self-interest, or prejudice, but in the light of collective wisdom gained on the job. The same wisdom permeates this pamphlet, which is an objective statement of facts from members working in comprehensive schools, with no comments or attempts to draw conclusions either way. What stands out is a selfless concern for the individual pupil, and a determination to make a success of enterprising experiments. Opinion is expressed only in the concluding paragraphs, which rightly say that the nation should be grateful that these experiments are in such good hands.

The Advancement of Science, Vol. XVI, No. 3, December 1959. (The British Association for the Advancement of Science, London.) 7*s*. 6*d*.

This issue includes more of the Addresses delivered at the York Meeting, including *International Air Transport Problems*, by Sir William P. Hildred, *Living with Science*, by the Countess of Albemarle, and a report of two conferences on Scientific Films. It also contains the Council's report, an impressive summary of the wide activities of the Association. The journal is in future to appear six times a year instead of quarterly; a very seemly rhythm, if one may say so.

Classical Dynamics. By R. H. Atkin. (London: Heinemann, 1959.) [Pp. ix + 273.] 30*s*.

The author has written this book ‘ in the belief that it is possible to learn Dynamics and answer examination questions at the same time ’. The book is likely to prove popular for the second of these purposes; it is perhaps a comment on examinations rather than on the book to say that the first purpose is less certainly fulfilled.

It is not entirely easy to decide for whom the book is written. The level is university, and there is a large collection of clearly worked examples, many taken from university examination papers, together with a copious supply for the reader to solve for himself. The difficulty, I found, is to decide what knowledge the student is expected to possess as he starts.

The opening chapter is called ‘ Historical Foundations ’ and seems to slip in, under the form of history, the basic principles that one would like to see elaborated systematically for those who are to ‘ learn dynamics ’. The lack of an index (other than paragraph headings) becomes catastrophic in this kind of treatment as one searches for definitions of words like *force, momentum, kinetic energy*. For example, what is the meaning of the phrase ‘ measures the energy which is latent in the field of force at $P(x,y,z)$ ’ on p. 35? (There is a definition of kinetic energy on p. 61.)

Some points of detail worried me. One appreciates that the work is Applied Mathematics, but the mathematical argument ought not for that reason to become slipshod. For example, the statement (p. 39)

$$‘\ |\mathbf{dr}| \to r \sin\phi\, d\theta \text{ as } d\theta \to 0\ ’$$

is no more (nor less) true than the statement that

$$‘\ |\mathbf{dr}| \to r \sin\theta\, (d\theta)^3 \text{ as } d\theta \to 0\ ’,$$

the result in all cases being zero. Again (p. 95), with

$$s=\frac{1+3\cosh nt}{5+3\cosh nt},$$

the argument ‘ $s \to 1$ as $t \to \infty$ so that the bead takes an “ infinite time ” to reach B ’ is incomplete; *s might* tend to 1 for some earlier, finite, value of t.

There seem to be one or two places, too, where the mechanical argument needs amplification. Thus (p. 156) the fact that the reaction R between a rolling sphere inside a fixed sphere is a function of θ which is positive at π does not of itself ensure that θ ever reaches the value π; the function $R(\theta)$ might become negative for a stretch between $\frac{1}{2}\pi$ and π returning to positive near the top. Also (p. 64) the equations

$$\ddot{x}+k\dot{x}=0, \quad \ddot{y}+k\dot{y}=-g$$

are used to cover rising *and* falling motion without further explanation; the earlier examples (p. 58) have been solved by different origins and senses of direction. (The beginner needs to be told why the analogous equations do not hold completely for resistance $\propto v^2$.)

I am, somewhat unwillingly, aware that this is not a wholly favourable review. Yet the book will undoubtedly be very acceptable. It covers a good range and is, usually, clear and helpful. I imagine that a second edition will be called for quite quickly. If, as I hope, that is so, I would plead for a thorough overhaul which will (i) expand Chapter III (Some Preliminary Ideas) to give a systematic account of the development of the really basic principles, (ii) include an index, and (iii), the book now being fully suitable for readers to tackle without help when that is not available, provide the answers to the examples.

E. A. Maxwell.

British Nuclear Reactors. By Gerard Gibbons and Kenneth Jay. (London: Chatto and Windus, 1960.) [Pp. 32.] 6*s*. 6*d*.

Starting with an elementary explanation of nuclear reactions and energy changes in general, this booklet shows what goes on at Calder Hall, the construction and purpose of Dimple and Dido and Dounreay, and the working of Zeus and Zephyr and Zeta. It is well written in simple language, admirably illustrated in colour, and shows a quite inspired gift for explanation. Fourth-form boys could understand it; sixth-form teachers may find it useful; and the general reader will be enlightened by it. The production is attractive, the price most reasonable, and the whole deserves the highest recommendation.

Materials and Processes of Electron Devices. By MAX KNOLL, assisted by B. KAZAN. (Springer, 1959.) [Pp. xii + 480.] DM 66.

The book on Werkstoffkunde der Hochvakuumtechnic written by Espe and Knoll in 1936 has long been a valuable source of information about the materials and processes of valve production and allied arts. Knoll's new book, in English, contains much of the same material though it has been brought generally up to date and some of the sections, particularly on metallurgical topics, have been considerably enlarged. There is a section on semi-conductors which describes in outline the preparation and some of the manipulation of pure germanium and silicon, there is however no account of modern transistors.

The vacuum technique described is essentially that of sealed-off electron devices, and the ' plumbing ' of dynamic vacuum systems is not discussed. There is a tendency to draw information from German and American rather than English or Dutch practice, and perhaps because of this, certain important processes (e.g. gold-wire compression seals for metals, frame-wound grids) have been missed.

The main adverse criticism of the book is that too much of its technical information goes back to the thirties, and its illustrations in particular have an old-fashioned look. It is not made apparent, for example, that pressed glass bases have very largely replaced pinch seals in small valves, or that argon-shielded arc welding is an important process. This criticism does not apply to the information about the properties of materials or fundamental processes, which is valuable and conveniently presented, with a wealth of references and tables.

Many laboratories will find this information invaluable. H. J. J. BRADDICK.

Beam and Wave Electronics in Microwave Tubes. By R. G. E. HUTTER. (D. Van Nostrand Co. Inc., 1960.) [Pp. xii + 378.] 73*s.* 6*d.*

A relatively short time ago, electromagnetic waves at frequencies of 3000 Mc/s and above were generated almost exclusively by reflex klystrons and magnetrons, and amplification at these frequencies was not really practicable. Now an additional large family of oscillator and amplifier tubes exists, all of the so-called ' travelling wave type '. As its title suggests, it is the purpose of this book to present the theory of these devices. The author, a chief engineer with Sylvania Research Laboratories, notes that the book was written partly as a textbook for *schools;* the mathematical analysis begins with Maxwell's equations and is therefore more appropriate to university and technical college courses in Britain.

A short introductory section has chapters presenting a statement of some high frequency effects in conventional tubes, an outline phenomenological description of the various types of microwave tubes and a résumé of the properties of simple (constant cross section) waveguides and cavities. Among the following more detailed analyses one may refer to the discussions of periodically loaded waveguides and, later, of electron beams in these structures.

This book gives a good mathematical account of its subject matter and as such is well recommended to the serious student. It is not suitable for one who seeks to dip and skip.
K. J. STANDLEY.

Space Flight. Volume 1: Environment and Celestial Mechanics. By K. A. EHRICKE. (London: D. van Nostrand Company, Inc., 1960.) [Pp. x + 513.] 109*s.*

This is the first of three volumes for the astronaut, written by the Director of the Centaur and Vega Programme of Convair. The whole is part of the series " Principles of Guided Missile Design ", a major publishing enterprise. It is written for specialists, and the question facing this reviewer is whether it can, at this price, be recommended as a reasonable investment for the ordinary science library. The first half of the book, in three long chapters entitled Prelude to Space Flight, The Utility of Space Flight, and The Solar System, is an encyclopaedic source of both historical and up-to-date information. The second half deals with central forces, orbits in space, and perturbations; this is for the mathematician. It is a first-class production, and I can answer my question indirectly by saying that once it had been acquired, the first 250 pages of the book would have a useful reference life. But pressed to a direct answer, I can only give a very reluctant " no ".

Static Electrification. By Leonard B. Loeb. (Springer-Verlag, 1958.) [Pp. xii+240.] DM 48.

This highly individual monograph is the only one at present available on the subject. It gives a very full account of the experimental investigations carried out in the author's laboratory at the University of California, with *ex cathedra* interpretations of the results, and it deals extensively with the electrification of liquids. The beautiful experiments of Kluge, the theory of Frenkel and the charging of textiles, however, find no place in the book, and the work of Natanson on the entrainment charging of liquids receives scant mention.

The theoretical discussion has not been brought really up to date. The modern theory of metals is used, but not the corresponding theory for insulators and semiconductors.

The author does not take a definite stand on whether the charging of insulators is normally due to the movement of electrons or of positive ions, but tends to assume that it is the former. He does support the view, which goes back to Volta and Helmholtz, that ' frictional ' electrification has very little to do with friction.

There is much to be learnt from this book which should be read by everybody seriously interested in static electrification, but it must be read critically. The production is of the superb quality one associates with the name of Springer, and the price corresponds. There is a good index and an extensive bibliography. W. R. Harper.

Theory of Elementary Particles. By P. Roman. (Amsterdam: North-Holland Publishing Company.) [Pp. xii+575.] (N.P.)

It takes an ill-informed man to define an elementary particle. Or a professional with nothing better to do. Although the book under review is about the theory of elementary particles the author wisely does not say what they are; the reviewer will not be as foolhardy as to try to remedy this deficiency. It is a question of point of view. If our only tool for the examination of the deuteron were slow neutrons (a few ev) we should count the deuteron as an elementary particle. But when we tap it a little harder (a few Mev) it disappears giving us a neutron and a proton. The summed mass of neutron and proton is only very slightly greater than that of the deuteron so we reasonably but unjustifiably conclude that the deuteron is not an elementary but rather a composite particle consisting of a neutron and a proton bound together with an energy small compared with the rest mass of either. Are the neutron and proton themselves elementary particles? We can tap them quite hard (tens of Mev) and all that happens is that they tap back unchanged. But tap them harder (hundreds of Mev) and out comes a copious supply of π-mesons. A little harder (thousands of Mev) and the nucleon becomes a cornucopia yielding π-mesons, K-mesons, hyperons of several kinds, anti-nucleons, anti-hyperons and, can we doubt it?, particles yet to be recognized. We also learn that the nucleon has an extension in space and we believe that different members of the galaxy of particles that it yields under duress are associated with different regions of that extension. But are they there before we tap? If so, they are bound together with an energy comparable with or greater than their own rest masses and their identities are lost in the definition of the composite particle, the nucleon. And they themselves? Is the π-meson to be regarded as a composite of a nucleon and an anti-nucleon which themselves in turn are pieces of machinery containing π-mesons? This last poses the supplementary dilemma: which particles shall be ' basic ' and which consubstantiate? Shall we follow Goldhaber-Györgyi and write $\Lambda = N\bar{K}$ or Sakata-Okun and write $\bar{K} = \Lambda\bar{N}$? *Quis custodiet ipsos custodes* up to date. And is there meaning to the problem? The distinction between truth and a successful heuristic device may be impossible to draw at the level of the elementary particles. (Students of human nature will notice a close analogy between the composite models of elementary particles and the characteristics of research terms in both theoretical and experimental elementary particle physics which are treated as units for purposes of discussion, scheduling, etc., and although apparently composites of identifiable persons show an individuality of their own and properties not to be found among their several constituent components.)

Further worries for those concerned with the definition of elementarity come from the mortality, transfiguration and transubstantiation of the elementary particles. They don't last, they turn into each other on collision and if left alone usually decay spontaneously into yet others in what is by our standards a very short length of time (say 10^{-10} seconds) but which by theirs is often such an age that it drives us to the formulation of new laws of nature. In one of the most beautiful hydrogen bubble chamber photographs ever taken (by the Alvarez composite in Berkeley) we find the transfiguration of a fast anti-proton and a proton into a Λ-hyperon and an anti-Λ-hyperon each of which is seen to transform itself spontaneously, the Λ-hyperon into a π-meson and a proton and the anti-Λ-hyperon into a π-meson and an anti-proton which later amalgamates with a proton and suffers transubstantiation into a spray of π-mesons each of which is to perish within a millionth of a second: reincarnation into a whole herd of Gadarene swine. In many ways elementary particles seem to fit more naturally into Eastern than into Western philosophies. That great meeting-ground of twains, the San Francisco Bay Area is also rich in elementary particles but it is with an unconscious appropriateness that Jack Kerouac leaves it to a character from Berkeley (" The Dharma Bums ") to give us the beatnik view of elementary particles: " '. . . All things are empty, ain't they? ' 'Whattayou mean, empty, I'm holding this orange in my hand, ain't I? ' ' It's empty, everything's empty, things come but to go, all things have to be unmade, and they'll have to be unmade simply *because* they were made! '. . . ' Your and your Buddha, why don't you stick to the religion you were born with? ' my mother and sister said." Also the shoemaker to his last and the physicist to his physics no doubt, for nothing engenders suspicion so much as a physicist who turns philosopher. However, this digression may have value in showing that the composite models of elementary particles are not likely to lead us to a definition of what we are talking about.

The composite models of elementary particles form the romantic approach to the problem. There is another, classical, approach and that is the algebraical. Here the various particles are characterized by different sets of quantum numbers; we hope that no two particles share the same set and that we can construct a closed scheme in which every possible place is occupied. This will then ' explain ' the elementary particles. The details such as relationships between mass values are to be understood in terms of postulated interactions between the particles or their underlying fields (also of postulated form) in such a way that everything is self-consistent. The fewer the interactions that are used to close the ring the happier we are. We are free to construct the scheme and to invent the field equations as we will: the approach is completely postulational.

The romantic and classical views are complementary and come together at many points but their approaches are quite different: the composite models almost give us the feeling that we can see the wheels go round; the postulational approach is likely to seem somewhat dry and, to the romantic, to be remote from what he dangerously clings to as ' reality '. The classical approach requires us to know, for example, that " every irreducible representation of an abstract finite semi-simple algebra is a faithful representation of a simple subalgebra " (from a paper on elementary particles by Bhabha written before the discovery even of the π-meson). This is a far cry indeed from Kelvin.

Insofar as Roman's book is interested in the problems of the representation of the elementary particles its approach is postulational; composite models are awarded a scant four pages. But the postulational approach is in fact our only way of ' getting anywhere ' since we have no tool other than field theory for the making of direct quantitative predictions. We will not go into the question of whether dispersion relations merely test indirectly certain of the bases of field theory or may provide a substitute for field theory or are simply a set of semi-empirical relations that bring together a wide range of experimental data; least of all will we be provoked by Chew's remark concerning the Mandelstam relations: " Perhaps they have no solutions at all except for certain definite values of the masses and coupling constants." It is interesting that Roman's book contains no mention of dispersion relations, perhaps the most vigorous corner of elementary particle theory today. The book is, in fact, concerned chiefly with those aspects of elementary particle theory that have the most general validity—the symmetry and invariance properties and selection rules that are implied by our most cherished concepts such as Lorentz invariance, and, on a slightly lower level, by our being wedded to field theory. It answers questions like: " What can we say about particles of spin $\frac{1}{2}$ without knowing their names? " or: " What type of mathematical machinery will be useful irrespective of the detailed properties of the particles it is used to treat? " This is a pedagogical text and provides a course of required reading for the white hunter in a dark land. There is an excellent discussion of the four-dimensional orthogonal group followed by two chapters on fields and their quantization. A long chapter on the ' classical ' symmetries C, P, T leads into a final chapter on isobaric space and its extension (nicely handled) to admit the ' strange ' properties of the K-mesons and hyperons. It is a restrained, orderly and valuable book, written to provide a set of tools; but it will not excite enthusiasm for other than the formal beauties. Neither will it, despite its author's expressed hope, excite many experimenters. Roman's sympathies for the plumbers is not close. Hear him towards the end of Chapter I. " It will perhaps be heart-lifting to conclude this long but certainly very important Chapter I with some ' physical ' considerations regarding the definition of spin." The experimenter presses on with lifted heart to read: " Consider a field defined over the points x_μ of the Minkowskian space-time world. Suppose that the components of the field, $\psi_\alpha(x)$, have a definite transformation property under the proper Lorentz group L_p, i.e. are covariant entities, spanning a representation space $\mathfrak{D}^{jj'}$ . . ." It's all semi-simple when you know how. D. H. Wilkinson.

Molecular Structure—the Physical Approach. By J. C. D. Brand and J. C. Speakman. (London: Edward Arnold (Publishers) Ltd., 1960.) [Pp. viii + 300.] 30*s*.

The authors of this book have attempted a very necessary task, namely, to write a general account of the physical methods for investigating molecular structure, ' at a level suited to the honours student, or to the research worker in some other branch of Chemistry '. This is also a very difficult task.

The plan of campaign is the following. After a brief introductory chapter, they acquaint the reader with the elementary ideas and notation of symmetry and of symmetry operations. Then they give the elements of wave mechanics and follow their application to the rotator and the vibrator, and to the absorption of radiation by such systems. Next there are chapters on the rotation of molecules, which includes a treatment of dielectric constants, and electric dipole moments, on nuclear resonance spectra (nuclear magnetic and nuclear quadruple resonance, and quadruple coupling), on the vibration of molecules and the analysis thereof, on the anisotropic polarisability and the Raman effect, on crystal structure analysis, and on electron diffraction by gases and vapours. Finally there is a chapter devoted to ' appraisement and conclusions '.

Of deliberate choice the authors have not dealt with electronic absorption and emission spectra, or with electron spin resonance spectra, or with magnetic properties derived from electron spin, or with studies of molecular energetics beyond those associated with rotational and vibrational energy levels. These decisions might be held to limit the scope of the book undesirably; but it keeps the size within bounds, and the topics which are treated form a reasonably coherent group.

An opinion about the authors' success in their aim depends upon the assessment of the knowledge, industry and needs of the honours student or of the research worker. The writing is excellently clear and the treatments seem reasonably rigorous and sound. The emphasis is on principles rather than on illustrating the applications, although illustrations are given.

A typical honours student decided that the book is too detailed and too purely physical for him. He wants less principles and more illustrations; and he did not find that the last chapter gave an adequate idea of the strong and the weak points of the methods. The chapter on wave mechanics could be omitted.

The research worker, having appreciated the effort that is necessary to master any subject, may be more willing to do the hard work required. The best honours men will probably do the same. To anybody willing to make the effort, this book is in many ways an excellent introduction. Criticisms in this respect, however, are that references to the original literature are not very numerous, and that the bibliographies at the ends of the chapters are not all as extensive as they should be.

The authors have made a very gallant attempt to solve the difficulty; so it is much to be hoped that a large number of readers will co-operate by girding up their intellectual loins.

The printing is excellent and the price moderate. L. E. Sutton.

Modern Physics. By H. A. Wilson. (London : Blackie & Son Ltd., 4th edition, 1959.) [Pp. xv + 473] 40*s*.

To those who are bewildered by the seemingly prodigious rate of growth of physics, it must come as a relief to find that H. A. Wilson's version of Modern Physics is still worthily holding a place on the university bookshelves some thirty years after its first appearance, altered in places but basically much the same as the original.

To a considerable extent, the longevity is a tribute to Professor Wilson's wisdom of choice of his material. For the rest, we must concede that there was little in the first edition which has become superseded. As the undergraduate's capacity for learning is limited, much of the newer material of these decades must remain post-graduate information.

To those unfamiliar with earlier editions of this book, it should be pointed out that the reader is presumed at the start to be familiar with the rudiments of electromagnetic theory and competent with a range of mathematical equipment ; thus on pp. 2-3 there is an outline derivation of Maxwell's equations in Heaviside-Lorentz units and with vector notation. This provides a firm basis for the excellent summary of Lorentz's electron theory of matter and results are deduced elegantly and rapidly. The author then leads the reader to see the inadequacy of the simpler concepts and introduces the new quantum mechanics and the various powerful tools of the art—including matrices, method of variations, perturbations and so on—persuasively and without fuss.

Having introduced the reader to modern lines of thought, Professor Wilson gives a lucid discussion of modern views on the intimate structure of the atomic nucleus, followed by interesting accounts of such topics as x-rays, conduction in gases, radioactivity, nuclear physics and so on. This culminates in the presentation of special and general relativity with a confident and disarming approach which most students will appreciate.

There is to some extent an anti-climax produced by the chapter on low-temperature physics, which was not part of the original conception of the book and does not sit easily in this position.

Those topics which have attracted full attention from Professor Wilson provide models of concise presentation and the undergraduate will feel for example that he is fairly grappling with Pauli's Exclusion Principle and is beginning to be conversant with Parity and on nodding terms with the deuteron. However, there is only skimpy reference to the literature available for further reading and in some instances even these references are out of date. Furthermore, where new material has been added to the text, it is not always consistent with the matrix into which it is supposed to fit, giving a patchwork effect. The word ' recent ' has persisted through various editions of this work and it hardly seems still appropriate to the pioneer work of Barkla on x-rays (including the J Phenomenon, which might now be given a quiet burial) and of Millikan and others on cosmic rays. This could sadly mislead students reading the subject for themselves.

Universities are so varied in standard and syllabus that any recommendation will be contradicted somewhere or other, but this book seems to us best suited for about the second year of a typical Physics Honours degree course. Many university teachers will see an advantage in the slowness with which successive editions transform this book ; but those who appreciate the penetrating and clear exposition which Professor Wilson gives of the topics on which he concentrates will regret that the same talents have not been employed on subjects of more recent growth or emphasis such as Solid State Physics.

The original format of this well-produced book is preserved and the price is modest by current standards. A. J. Woodall, C. G. Wilson.

Scientific and Engineering Manpower in Great Britain, 1959. Report of the Committee on Scientific Manpower of the Advisory Council on Scientific Policy. (Cmnd. 902, H.M.S.O., November 1959.) 3*s*.

This Report provides information about the numbers and employment of scientists and engineers in Great Britain at the beginning of 1959, and considers the changes that have occurred since 1956. It also includes a review of employers' forecasts of their demand for scientists and engineers by 1962. No reassessment has been made of the long-term demand dealt with in the previous Report of 1956, but consideration has been given to the way in which the relationship between supply and demand might develop during the next three years.

Training in Radiological Health and Safety : Report of a Committee appointed by the United Kingdom Atomic Energy Authority. (London : H.M.S.O., 1960.) 5*s*. 6*d*.

This Committee, under the chairmanship of Sir Douglas Veale (formerly Registrar of the University of Oxford) was set up to consider the national need for specialists to deal with the health and safety aspects of the use of ionising radiations and radioactive substances and the report makes recommendations about the types of experts needed and their training.

The report describes (Chapter II) the types of work involved in achieving radiological health and safety, and the kinds of training needed. It then reviews (Chapter III) the present position in this country and discusses the responsibilities in the health and safety field of government departments, local and public authorities, hospitals and the medical profession, and nationalised and private industry. An attempt is made to estimate the total number of trained people required during the next ten years.

Methods of training for radiological health and safety staff at three levels are considered (Chapter VI). At the top level university postgraduate courses have already been set up to give a wide training—overlapping the boundaries of medicine, biology, physics and chemistry—to selected graduates in science or medicine. There are courses in the Universities of London and Birmingham, and Edinburgh University is planning a course to start next October. The Committee believe that these courses, when fully developed, will be sufficient to meet the need for twenty or thirty top grade specialists a year.

The report recommends that similar courses of a rather less advanced kind should be set up at two or three colleges of advanced technology (Chapter VI). At a somewhat lower level again, a scheme is suggested for introducing certificate courses at technical colleges in the autumn of 1961 (Chapter VI).

The idea of a national training centre is examined (Chapter V), but the Committee consider it unnecessary, and recommend that the existing educational system should be used to provide the sort of training that is needed.

To make the best use of specialists, the Committee recommend (Chapter IV) that a national radiological advisory service, with regional centres, should be set up under the Minister for Science. This service would be able to give advice to organisations (such as firms making use of radioisotopes) without full-time health and safety specialists of their own. It would be available to give expert help in the case of an accident involving possible radio-activity hazards. It would also provide such services as processing the film badges which are worn by all workers who may be exposed to radiation.

The report also recommends (Chapter VII) that all medical students should be given some knowledge of radiological health and safety in their undergraduate training, and that a course should be included in the curriculum for the Diploma in Public Health.

Short residential courses for managers, administrators, medical officers of health and other professional workers are also proposed, as well as books, films, articles, and radio and television programmes for the information of the public (Chapter VII).

Biographical Memoirs of Fellows of the Royal Society. Vol. 5. (London: The Royal Society, 1960.) [Pp. 269.] 30*s*.

This volume records the biographies of some nineteen Fellows who died during 1959, many of whom won distinction in various fields of physics. T. L. Eckersley's work on the ionosphere is described by J. A. Ratcliffe. S. R. Milner's wide range of interests is discussed by T. E. Allibone and J. R. Clarke, with a note by D. Gabor on his work on Eddington's E-numbers. Wolfgang Pauli's impact on theoretical physics and on apparatus is discussed by R. E. Peierls in a lively personal account, and W. Wilson takes us back to the early days of modern physics in writing of the long career of Sir Owen Richardson. D. S. Kothari pays tribute to Meghnad Saha. And A. C. Menzies describes Frank Twyman's work on interferometers and on the improvement of optical glass, and his active interest in the education of apprentices.

Elementary Particles

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1. Introduction

The history of science is the history of search for unifying concepts. The last fifty years of physics have been mainly occupied with the search for, and identification of, the 'ultimate' constituents of matter and energy, and with the development of concepts needed to understand their mutual interactions. This brief survey will attempt to give an outline of this development.

2. Momentum and energy

The major achievements of nineteenth-century physics were the discovery that all matter, of whatever form, is composed of up to ninety-two different types of atoms, the atoms of H, He, Li, Be, B, C, etc., and the realization that, barring the energy of motion and gravitational energy, all other forms of energy whether existing as heat, light or chemical energy are basically electrical in character.

At the dawn of this century Einstein made the still deeper synthesis when he asserted that all energy possesses inertia, that matter and energy are completely interconvertible, and that both are manifestations of the same fundamental entity. Einstein's assertion was important but still more important was the manner of the reasoning which led him to it. Based on his postulates about the structure of space and time, this reasoning contains practically all ingredients which have later gone into the making of elementary particle theory.

Einstein's Special Theory of Relativity started with the astonishing experimental observation that the velocity of light as measured by an experimenter is independent of any velocity he himself may possess—that the velocity of light is an 'invariant' quantity. Einstein's first insight was the recognition of the fundamental role of 'invariant' quantities for description of physical phenomena. He showed that the constancy of light-velocity in turn implies that neither the space-distance $\boldsymbol{r}$ between two neighbouring happenings, nor the time-interval t between them possesses any invariant significance. Instead, he showed, that the only entity which has the same numerical value for all observers irrespective of their own motion is the (squared) space–time distance which he defined as $s^2 = r^2 - \mathbf{c}^2 t^2$. In essence then, space and time are aspects of one fundamental entity. Now insofar as the momentum and energy of a freely moving particle are physical quantities, originally defined by Newton and his eighteenth-century followers to possess the same 'transformation character' as space r and time t respectively, one may likewise expect energy and momentum (when properly defined) to form two aspects of one basic (co-variant) entity. The basic invariant here which corresponds to 'space–time distance' is the rest-mass of a particle. This is the entity which has the same value for all observers. More precisely if $\boldsymbol{p}$ is the momentum and E the energy

$$E^2 - \mathbf{c}^2\boldsymbol{p}^2 = +m^2\boldsymbol{c}^4$$

where m is the (constant, invariant) rest-mass of the moving particle. It was Einstein's insistence on the 'transformation character' of a physical entity as a guide to its proper definition which marks the beginning of modern physical theory.

3. The electron, the proton and the photon

Returning to the discovery of elementary particles, the subject began with the discoveries of the electron (1897) by J. J. Thomson and the proton (1911) by Rutherford. However, around 1914, with the years of intense activity at the Cavendish Laboratory, the subject also seemed to have come to the end, for it then appeared that all matter—all the 92 atoms—were made from just these two stable, indivisible indestructible entities. The two particles themselves have the following properties:

(1) The electron is a tiny chunk of matter with a rest-mass of 10^{-27} g, the proton has a mass some 2000 times as large.

(2) The electron carries a negative charge $4{\cdot}8 \times 10^{-10}$ e.s.u., the proton a positive charge of exactly the same magnitude.

To this list was soon added the third elementary particle, the photon. Planck and Einstein recognized that the radiation energy of an electromagnetic field exists in the form of discrete chunks, discrete quanta, which Einstein called photons. In this (almost Newtonian) view a beam of light consists of a stream of particles, the photons, all travelling with the velocity of light.

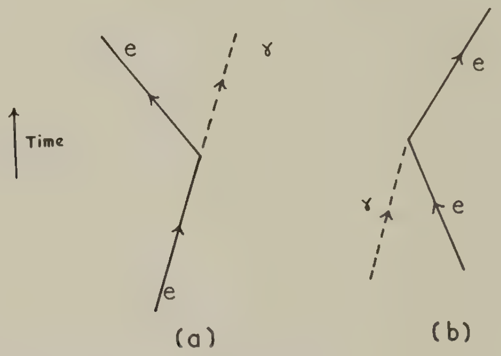
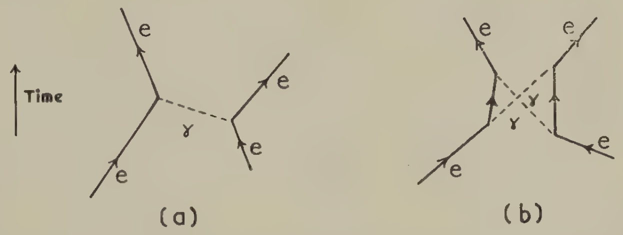


Fig. 1. (*a*) Emission and (*b*) absorption of a photon by an electron.

Since all our concepts in elementary particle physics are modelled on the pattern set by these three particles, the electron, the proton and the photon, let us consider their interactions in some detail.

In accordance with Maxwell's electromagnetic theory, all charged particles, when accelerated by electric fields, emit or absorb electromagnetic radiation. According to Planck and Einstein, electromagnetic radiation exists in the form of photons. This means that, while they are accelerating, electrons or protons emit or absorb photons. We can (fig. 1) make a space–time diagram of this emission or absorption process, in fig. 1, on the left, an electron (solid line) is emitting a photon (dotted line); on the right an electron absorbs a photon.

In Maxwell's theory, one charged particle attracts or repels another charged particle by first producing an electromagnetic field in the surrounding space which in turn acts on the second charged particle. This will now be pictured as an emission of one or more photons from one electron and their reabsorption by the second (fig. 2).



(a) (b)

Fig. 2. Repulsion between electrons by the emission and reabsorption of photons; (*a*) a single photon, (*b*) two photons.

The emission or absorption of photons and their exchange between two electrons or an electron and a proton must satisfy certain conservation laws:

(i) In any physical process
  (*a*) the total charge (Q)
  (*b*) the number of electrons (N_e)
  (*c*) the number of protons (N_p)

must remain the same before and after the interaction. Since photons are freely emitted and absorbed by electrons and protons, and their numbers can alter during the course of an experiment.

(ii) In any physical process
  (*a*) the total energy (E)
  (*b*) the total momentum ($\boldsymbol{P}$)
  (*c*) the total angular momentum ($\boldsymbol{J}$)

must remain the same before and after the interaction†.

These so-called conservation laws are empirical laws. The first three assert that protons and electrons are indestructible entities. The last three possess important implications for our ideas about the structure of space–time. The conservation of energy and of linear momentum implies that the results of an experiment are independent of where in space and when in time it is performed. That is, space and time are *translation symmetric*. From the conservation of angular momentum we may deduce that if the experimental set-up is rotated through any angle, there is no effect on the observations.

This is the principle of *rotation-symmetry* of space.

One might of course have started with the plausible assumption that the results of an experiment do not depend on where it is performed, that space–time structure

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† The precise statement of conservation of energy and momentum is the following. Consider an electron–proton collision. Let m_e be the electron rest mass, $\boldsymbol{k}_e$ its momentum before and $\boldsymbol{k}_e'$ its momentum after the collision and let m_p, $\boldsymbol{k}_p$ and $\boldsymbol{k}_p'$ be the corresponding quantities for the proton. Then

$$\boldsymbol{k}_e+\boldsymbol{k}_p=\boldsymbol{k}_e'+\boldsymbol{k}_p'; \quad \sqrt{(m_e^2c^2+k_e^2)}+\sqrt{(m_p^2c^2+k_p^2)}=\sqrt{(m_e^2c^2+k_e'^2)}+\sqrt{(m_p^2c^2+k_p'^2)}.$$

is translation and rotation-invariant, and then deduce that the total energy and linear momentum, also angular momentum, must be conserved in any given experiment. In a sense, then, elementary particle physics at this stage is teaching us the correctness of the basic space–time notions of Einstein's special relativity.

4. The Dirac electron

To give the law of angular-momentum conservation its full qualitative effect it was found necessary to ascribe an intrinsic angular-momentum, usually called an intrinsic *spin*, to each of the elementary particles. The spins assigned are: 1 unit to the photon, $\frac{1}{2}$ unit to the electron, and $\frac{1}{2}$ unit to the proton.

The unit equals $\boldsymbol{h}/2\pi$ where $\boldsymbol{h}$ is Planck's constant; it is often written $\hbar$.

To fix one's ideas, one may picture the electron or the proton rather like a spinning top. The spin axis is not fixed in the particle. But the appropriate axis usually suggests itself—thus if we have a moving electron travelling with momentum $\boldsymbol{p}$ the direction of $\boldsymbol{p}$ is a suitable reference axis. So, in such a beam of free electrons, half the electrons will be found spinning in a right-hand screw sense relative to the vector $\boldsymbol{p}$, and half in the left-hand screw sense.

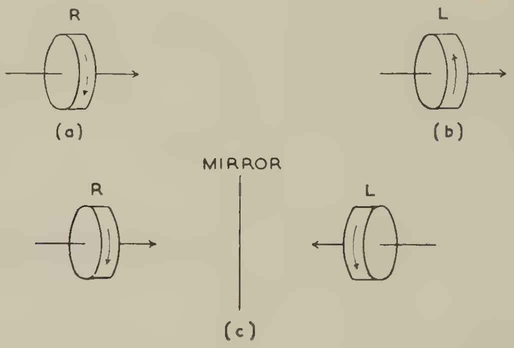

Fig. 3. Polarization, with respect to an axis indicated by the long arrow (*a*) right-handed polarization, (*b*) left-handed polarization, (*c*) left-handed polarization is the mirror-image of right-handed polarization.

Clearly (fig. 3) a right-spinning or a right-polarized electron is a mirror-image of a left-spinning or a left-polarized electron.

To go back to the relation of the angular-momentum conservation law to space–time structure, Dirac was able to show in 1928 that intrinsic spins of elementary particles and particularly the half-integer spins of electrons and protons arise naturally when one considers space–time (or Lorentz) rotation-symmetry rather than merely space-rotation symmetry.

Dirac started by trying to write the equation of motion of a free electron or a proton. There was only one guiding criterion for Dirac; the equation should be (Lorentz) invariant, i.e. the intrinsic properties of the particle should appear the same to a moving or to a stationary observer. Dirac succeeded in writing such an equation. The first surprise came when he found that the particle which the equation described must carry an intrinsic spin of half a unit although he never started by looking for a spinning particle. But there was also a second surprise. The equation described not only electrons (or protons) for which it was designed; it also seemed to describe particles of the same mass and same spin as electrons but carrying negative energy (i.e. particles of momentum $\mathbf{p}$ and energy $E = -\sqrt{(\mathbf{p}^2\mathbf{c}^2 + m^2\mathbf{c}^4)}$ instead of $E = +\sqrt{(\mathbf{p}^2\mathbf{c}^2 + m^2\mathbf{c}^4)}$. Such particles had never been seen before in nature; these particles when pushed *away* would travel *towards* the experimenter. In an electric field designed to accelerate electrons, negative energy electrons would be decelerated (because of their negative *energy*). Dirac's equation, like the curate's egg, seemed good only in parts.

For a number of years there was a conspiracy of silence about these unpleasant solutions of Dirac's relativistic equation. In 1934 Dirac himself suggested a way out. He proposed accepting all solutions of the relativistic equation as representing reality. Negative energy electrons should exist as much as positive energy electrons do. In fact there might be such an abundance of these particles that in a normal state of the Universe every conceivable state of negative energy might be filled with negative energy electrons. Dirac called this (unobservable) substratum of negative energy electrons the negative-energy 'sea'. Consider now the situation when one of the electrons in the 'sea' absorbs a (positive energy) photon, such that its total energy after photon-absorption is positive. It would then be seen as a normal positive energy electron. In making the transition, however, it will have left a vacancy, a hole in the negative 'sea'. This hole, connoting absence of negative energy as well as absence of negative electronic charge, implies the presence of a positively charged, positive energy particle with the same mass as the electron. The hole is a 'positron' decelerating as it should in an electric field which would accelerate normal negatively charged electrons, this time because it has a positive charge.

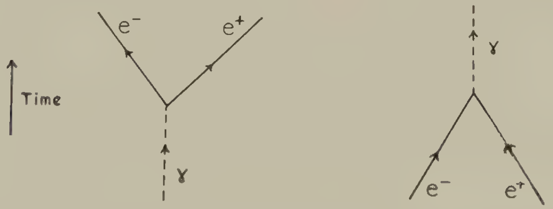

Fig. 4. (*a*) Electron–positron pair produced from a photon; (*b*) electron–positron pair annihilating with emission of a photon.

It is important to realize that in Dirac's picture it is impossible to create a hole (a positron) without also at the same time creating a normal electron (the electron expelled from the hole). Conversely the collision of an electron and a positron could mean filling up of the hole in the negative energy sea so that both the electron and the positron must simultaneously disappear. The space–time diagrams of fig. 4 show this.

Dirac's 'sea' like some of the space–time diagrams above was a picturesque descriptive device. The essence of his work lay, however, in the important discovery that *as a consequence of relativistic invariance*, all particles in nature must exist in pairs; that to every particle there corresponds an anti-particle of precisely the same mass and same spin, but opposite charge. Thus the existence of a negative electron implies the existence of a positive electron ; if the proton exists, so must an anti-proton. In fact the existence of the hydrogen atom implies that an atom of anti-hydrogen can exist with precisely the same energy levels.

This work of Dirac was some of the most momentous in the history of physics. It revealed a deep symmetry in nature, it provided a mechanism by which electron–positron pairs could be created or simultaneously annihilated. Of course we must modify the two conservation laws $N_e = \text{constant}$ and $N_p = \text{constant}$ to read

(i) $N_e - N_{\bar{e}} = \text{constant}$,

(ii) $N_p - N_{\bar{p}} = \text{constant}$,

where $\bar{e}$ and $\bar{p}$ stand for positron and anti-protons. We shall see later that these laws apply respectively to all kinds of *leptons* (spin $\frac{1}{2}$ particles of low mass like the electron) and all kinds of *baryons* (heavy spin $\frac{1}{2}$ particles like the proton), the general terms may as well be introduced here. From henceforth I shall call law (i) the law of conservation of leptons and law (ii) the law of conservation of

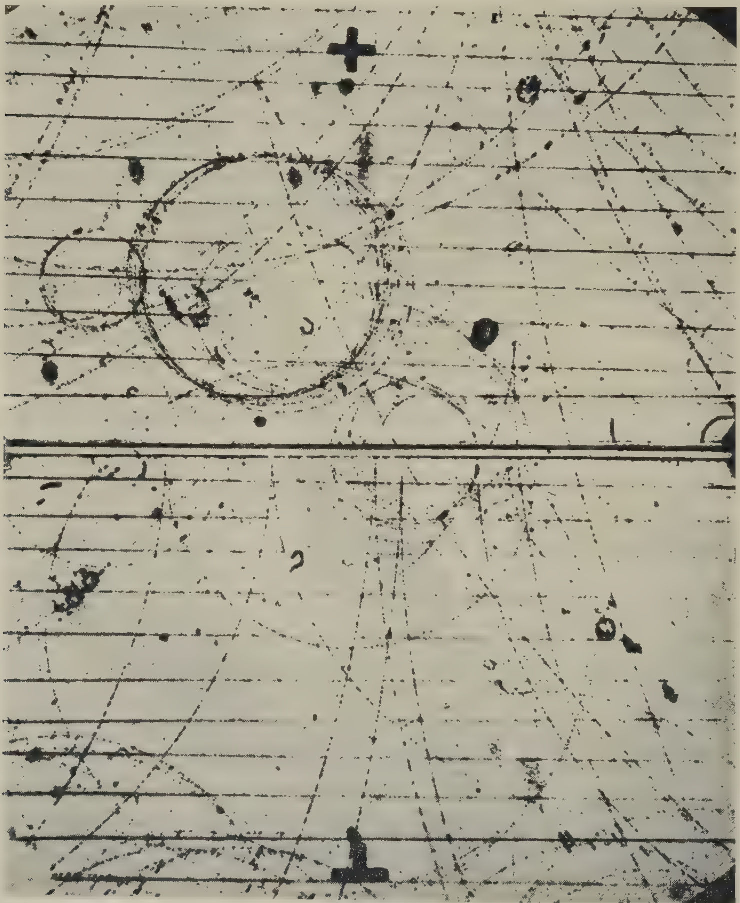

Fig. 5. Cloud-chamber tracks of electron-pairs generated in the lead sheet LL dividing the chamber, by γ-radiation incident from the upper side. Four pair-tracks, starting as inverted V's in LL can be seen.

baryons. The brilliant discovery by Anderson and Blackett in 1932 of electron–positron pair creation by cosmic-ray photons actually antedated Dirac's ideas and provided a confirmation of the law of particle–anti-particle symmetry. Figure 5 shows pair creation by photons when passing through a lead plate placed inside a cloud chamber. The photons being uncharged leave no tracks. One can, however, see the pairs of particles, the electrons curving to the right and positrons to the left. To make these oppositely charged particles curve in opposite directions the cloud chamber was placed in a strong magnetic field.

Dirac's work is such a landmark in elementary particle physics that considerable space has been devoted to it. One of the important aspects of its success is its demonstration that if one starts with correct physical ideas, one must then believe in all the mathematical consequences which flow from one's work.

There is an apocryphal story in Cambridge about how Dirac got the idea of interpreting holes in a negative energy sea as real physical particles. As a student, the story goes, Dirac once participated in the Christmas Problems Drive, annually organized by the Cambridge Students' Mathematical Society, the Archimedians. One of the 'quickies' was the following:

> Three fisherman go fishing one dark and stormy night; they make a haul and take it to a desert island to sleep the night off. In the middle of the night, the storm subsides and one of them decides to take his one-third and leave. He did not want to wake the others; he found he could divide the catch into three equal parts, with one fish outstanding. Throwing this fish into the sea and taking his one-third he left his sleeping companions. Presently the second fisherman got up; he did not know his friend had left. He started the sharing out process again; he likewise divided all fish into three equal parts, again he found he could do it with one fish outstanding. Throwing this odd fish into the sea he too departed with his portion of the catch. The same thing happened a few hours later with the third fisherman; he again divided the remaining fish into three equal parts and found one fish outstanding. The problem was, find the minimum number of fish with which this is possible.

Dirac's answer was -2 as the minimum number of fish. For surely -2 can be divided into three equal shares of -1 fish each with $+1$ fish outstanding. After throwing the outstanding fish $(+1)$ into the sea and collecting one's share $(-1$ fish) the balance is -2 again for the second fisherman to start dividing and so on.

5. SOME PROBLEMS OF SIZE

The spectacular success of Dirac's work produced a tremendous sense of well-being. True enough there were still some awkward questions which needed answering. The experimental fact that in any physical process light particles or heavy particles are never singly destroyed or created meant that these particles are in a sense indestructible. The problem of looking for further sub-units of which these particles might themslves be made seemed settled. However it remained an unanswered question, whether electrons and protons should be considered as point particles or as structures with a definite size, like for example tiny charged spheres. The structure of a particle is intimately bound up with its rest mass. If the electron is a charged sphere of radius a, the electrostatic energy stored up in building this structure would be roughly of the order of e^2/a. One might interpret the electron's rest-mass as the (inertial) manifestation of this stored energy obtaining (from the equation $e^2/a \sim mc^2$) a value for the radius $a \sim 10^{-13}$ cm. This might solve the problem of the electron's mass, but it would leave a new unresolved dilemma. If the electron is indeed a charged sphere, why does it not explode on account of the electrostatic repulsion of various parts of it? What provides the (extra) cohesive force which keeps an electron together?

Dirac's theory had ignored the problem of the structure of an electron completely; in the sense used above it is the theory of a point electron ($a \rightarrow 0$) with conceptually an infinite self-mass. With the success of this theory in accounting for most experimental results, it became even harder to understand how one might reconcile oneself to the unpleasant feature of an infinite self-mass.

The period between 1947 and 1949 was occupied in theoretical physics with a re-interpretation and re-evaluation of precisely these self-energy problems. Activity was stimulated by the discovery of the Lamb Shift, and the great theoretical triumph of this period was the recognition that a small shift in the 2S and 2P levels of the hydrogen atom arose because self-energy effects modified Coulomb's law at short distances (order 10^{-13} cm). But this fascinating story is long and somewhat unrelated and may be told elsewhere.

But what about an experimental determination of electron and proton radii? Let us recall how Rutherford was able to show the existence of small central nuclei inside relatively diffuse structures like atoms. Rutherford was led to the result that the positive charge of an atom occupies only a very tiny volume inside an atom by his experiments on scattering of α-particles by atoms. Rutherford had found a disproportionately large number of α-particles scattered backwards, as if "cannon bullets had been turned backwards when shot at pieces of paper". These scattering experiments gave a clue to the order of nuclear sizes. Clearly to find the size of the proton itself one should try something similar; such as scattering energetic electrons from protons. These experiments have recently been completed at Stanford by Hofstadter and his collaborators; they seem to show that the proton has indeed an enormous charge-radius of the order of 10^{-13} cm. Where does this 'large' radius come from? Why should the proton possess a 'massive' cloud of charge? For an understanding of this we must look elsewhere beyond electrodynamics.

6. Neutron decay: the neutrino

This 'elsewhere' came from nuclear physics. We have mentioned before that the electron–electron, electron–positron or electron–proton forces can be completely understood quantitatively on the assumption that these particles when close together exchange photons between themselves.

The next development in elementary particle physics came with the realization that the proton–proton force is only partly explained by the above picture. There seems a much stronger force between two protons when these are closer than 10^{-13} cm, stronger than the electro-magnetic force by a factor of 100. At about the same time came the discovery of the neutron, our sixth elementary particle, and the realization that all atomic nuclei contain about as many neutrons as protons. The neutron is nearly as massive as the proton; further, the proton–neutron force is equal to the proton–proton force to a very good approximation. The neutron and the proton could in fact be considered two states (charge-less and charged) of the same basic particle.

The neutron was destined to enrich the subject in a very different and surprising way. The neutron is slightly heavier than the proton, and in about 12 min a free neutron decays into a proton and an electron, the so-called beta-decay. This decay did not seem to conserve energy, momentum, or angular momentum. Here indeed was a searching test of the theoretical physicist's faith in the concepts he had himself created. If one gave up the demand that these conservation laws should hold, one would have to revise one's ideas about the structure of space–time. To resolve this dilemma Pauli (in a letter) suggested that in neutron decay, besides the proton and the electron, a neutral particle of zero rest-mass must be emitted, which carries away the missing energy, and missing momentum and the missing angular momentum. This particle was christened the neutrino. The discovery of the neutron then gave us two new elementary particles, the neutron itself (n) and the neutrino. The neutrino is represented by the symbol $\bar{\nu}$ or ν, the distinction between the two being discussed below.

But is the neutron an elementary particle? The particles we have dealt with so far, the electron, the proton, the photon and even the neutrino are all stable, indivisible and indestructible. This is not true of the neutron. To be sure, on the nuclear time-scale, the neutron decay is a very slow process. It can be shown by a simple application of the uncertainty principle that the type of space–time event which we have dealt with—like the emission and absorption of a photon (fig. 1)—takes place in times of the order of 10^{-19} sec. On this time-scale, the neutron decay, taking as it does some 10^3 sec, is a very slow process indeed. But all the same the fact of the decay remains, and forces us into a compromise. The compromise is the following: We divide all fundamental interactions into three classes:

Classification of interaction

	1. Strong	2. Electromagnetic	3. Weak
Comparative strength	1	10^{-2}	10^{-12}
	$\left.\begin{array}{l}\text{p–p}\\ \text{p–n}\\ \text{n–n}\end{array}\right\}$ forces	$\left.\begin{array}{l}\text{p–e}\\ \text{e–e}\end{array}\right\}$ forces	$\text{n} \to \text{p} + \text{e}^- + \bar{\nu}$ decay

(1) *Strong nuclear interactions*, which give rise to proton–neutron (p–n), proton–proton (p–p), and neutron–neutron (n–n) forces. These interactions are the strongest we know of in nature.

(2) *Electromagnetic interactions*, which give proton–electron (p–e) and electron–electron (e–e) forces. These forces are medium strong.

(3) *Weak interactions*, which are responsible for the decay of the neutron and for other processes to be mentioned shortly.

The relative strengths of these interactions are in the ratio of $1:10^{-2}:10^{-12}$. To the extent that the weak interactions can be neglected, the neutron is elementary, stable and indivisible. To the extent that the electromagnetic interaction can be neglected the neutron and the proton are identical. Thus all particles are elementary, but some are more elementary than others.

7. Mesons

This brings us to 1935 when Yukawa started to ponder over the problem of the specifically nuclear force, the p–n force. We have seen that the electromagnetic interaction of charged particles can be represented as arising due to an exchange of protons. Yukawa argued, that in complete analogy with this the proton–neutron force must also be produced by an exchange of some new particles which he called mesons. To give a simple analogy, consider two persons walking along the roadside. Let us suppose that they have a cricket-ball which they must go on exchanging between them. This circumstance will constrain them to remain within a certain distance of each other. An observer who cannot see the cricket-ball will attribute the fact that these two always remain close to one another to a friendly feeling between them, to an attractive force. From the characteristics of the nuclear forces, Yukawa deduced that the mesons must possess mass, and should be† about 300 times as massive as electrons; and also that (unlike photons), mesons must be charged as well as neutral.

As to the behaviour of mesons, like photons they would be emitted or absorbed singly by the protons or the neutrons and under suitable circumstances one such meson particle could create a proton–antiproton or neutron–antineutron pair. Conversely a nucleon could annihilate with an antinucleon, the energy and momentum being cancelled away by mesons.

The discovery in 1947 of these particles by Powell was a landmark in the theoretical understanding of elementary particles, and was equally a landmark in experimental physics. For it was made possible by the development of a new technique, by the opening up of a 'new window on nature'. The technique consisted of using photographic emulsions to register tracks of charged particles.

Coming back to our classification of interactions, let us consider strong interactions in more detail. At this stage the particles we are concerned with are:

(1) Two nuclear particles or nucleons (p, n) and their anti-particles.

(2) Three π-mesons (π^+, π^0, π^-), positive, neutral, and negative.

As remarked before, the forces are much stronger than electromagnetic forces. Thus to one part in a hundred, charge is irrelevant in nuclear physics. To the extent that electromagnetic forces can be neglected the proton and the neutron are identical particles. Similarly π^+, π^0, π^- are identical particles. The fact that these three π-mesons are equivalent to each other immediately suggests that we should formally treat these three as components of a vector in a [3] space. To distinguish between this abstract space and ordinary space we call this new space the isotopic-space. The equivalence of the three π-mesons is then formally stated as rotation-symmetry in the isotopic-space. As previously noted rotations are connected with angular momentum and we can, in analogy with ordinary space, assign an intrinsic isotopic spin I_z to the π-mesons. π^+ corresponds to $I_z = +1$, π^0 to $I_z = 0$, π^- to $I_z = -1$. That is, we are now speaking as if of three *states* of one kind of particle, THE π-MESON. Continuing the analogy to THE NUCLEON which can be recognized in two equivalent states (the proton and the neutron), we may assign $I_z = +\frac{1}{2}$ to the proton and $I_z = -\frac{1}{2}$ to the neutron. Just as in ordinary space rotation-symmetry implies conservation of angular momentum, so rotation-symmetry in isotopic-space implies conservation of isotopic spin.

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† The argument for deducing meson mass runs roughly as follows. The possibility of a meson's exchange implies that the proton or the neutron energy has an uncertainty of the order of meson mass m_π. From the uncertainty principle the characteristic time corresponding to this uncertainty in energy would be $\hbar/m_\pi c^2$. During this time the meson would travel a distance of the order of $\hbar/m_\pi c$ and this distance should equal the range of nuclear forces. From the known value (10^{-13} cm) of this range ($10^{-13}\,\text{cm}=\hbar/m_\pi c$) one deduces that $m_\pi \sim 300 m_e$.

In any nuclear interaction, then, not only energy, momentum, angular momentum, but also isotopic spin should be conserved. We shall be more precise about this shortly. Clearly the electromagnetic interaction, insofar as it distinguishes charged particles from neutral particles, and erases our capital THE, upsets the whole scheme and violates rotation-symmetry in this space.

Three new particles have now been added to our list. The same question as for the neutron arises again. Are these stable particles? The answer is no, and the decay of all these three particles presents novel features.

Consider π^- first. One might have expected that this particle would decay into an electron and a neutrino. A π^- could convert virtually into an antiproton neutron pair, and this pair would then annihilate giving an electron and a neutrino. For some most inexplicable reason this decay happens very infrequently†. Instead nature completely confounds us. The π-meson decays into a new particle (the muon) and a neutrino. This new particle, the muon, is about 200 times as heavy as the electron. The strength of this decay interaction is identical with the interaction responsible for neutron decay.

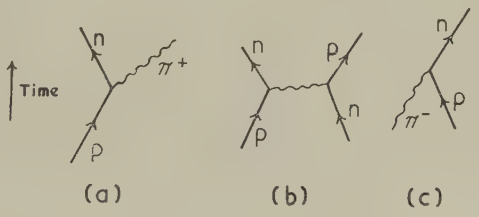

Fig. 6. Meson–nucleon interactions; mesons are represented by the wavy lines. (*a*) proton emitting π^+-meson, becoming neutron, (*b*) interaction between proton and neutron by exchange of a meson, (*c*) absorption of π^--meson by proton, giving a neutron. Notice the similarity between the meson–meson interactions and the photon–electron interactions of figs. 1 and 2.

The μ-mystery does not stop here. In about 10^{-6} sec the muon itself spontaneously decays into an electron and two neutrinos. Quantitatively once again it seems to be the same interaction which is responsible for the π-decay. We shall later see how the muon has figured prominently in the news recently. Without hesitation one may say that the muon is the most mysterious particle in physics. We do not know any good reason why it should exist nor do we know why it should have such a large mass.

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† Experiments at CERN Geneva showed in 1958 that $\pi \rightarrow e + \nu$ decay is not altogether absent but that it takes place with a frequency 10^{-5} smaller than of $\pi \rightarrow \mu + \nu$ decay.

The π^0-meson decays into two γ-rays in about 10^{-17} sec. This decay falls into a class by itself. Whereas the neutron decay and $\pi^\pm$, $\mu^\pm$ decays fall into the weak interaction category, π^0 decay is brought about by the medium-strong electromagnetic interactions and this is the reason for its correspondingly shorter life-time. Whereas n, $\pi^\pm$, $\mu^\pm$ would be stable particles if weak decay interactions were switched off, the π^0 would be stable only if the electromagnetic field was also missing. Once again some particles are more elementary than others. Would it be profitable to consider the π^0 as a compound made from two γ-rays? The answer is no, so far as the nuclear interaction is concerned. For the nuclear interaction of the π^0 is essentially as elementary as π^+ and π^- and the three always go together.

8. Hyperons and strange particles

Let us pause to review the situation once again. In 1947 we had a list of fourteen particles: γ, e$^\pm$, $\mu^\pm$, ν, $\bar{\nu}$, p$^\pm$, n$^\pm$, $\pi^\pm$, π^0. The pristine simplicity of the early days (before 1932) when only γ, e$^\pm$, p$^\pm$, were experimentally known (or suspected) was completely lost. We were faced with the problem of two more elementary forces of nature, the nuclear force and the explosive decay force; we had to reconcile ourselves to decaying elementary particles; worse still, we were faced with the dilemma of approximate conservation laws (the conservation of isotopic spin) and to cap all mysteries we must live with the mysterious muon.

After 1947 came still more surprises. Following a brilliant (and so far as theory is concerned completely unexpected) discovery by Butler and Rochester, eight new particles have been discovered and we turn to these. These particles fall into two distinct categories:

There are six particles called hyperons, heavier than the proton, Λ^0, $(\Sigma^+, \Sigma^0, \Sigma^-)$ and (Ξ^0, Ξ^-). All these are unstable and decay into a proton or a neutron, the decay force again being quantitatively the same as that responsible for n, $\pi^\pm$, and $\mu^\pm$ decays. Since a proton or a neutron is one of the end-products in the decay, the conservation law for baryons still holds. It now, however, reads

$$N_p + N_n + N_\Lambda + N_\Sigma + N_\Xi - (N_{\bar{p}} + N_{\bar{n}} + N_{\bar{\Lambda}} + N_{\bar{\Sigma}} + N_{\bar{\Xi}}) = \text{constant.}$$

In a second category are two particles, with masses intermediate between the nucleon and the π-meson. These decay after various stages, finally into electrons and neutrinos. These are the K^+, K^0 particles.

The discovery of Λ^0 and K^0 and Ξ^- particles was made in Wilson cloud chambers; the charged hyperons and K-mesons have been most extensively investigated using photographic emulsions while the confirmation of the existence of the Σ^0 and the recently announced Ξ^0 has come from 'bubble chambers' exposed to the proton beams of Brookhaven and Berkeley accelerators. The accelerated protons strike (liquid) hydrogen nuclei in the bubble chamber, all details of the particle tracks being recorded by the ion trails left in the wake of the particles as these travel in the chamber.

These eight particles presented certain unexpected features and for quite a number of years were called strange particles. From the copiousness of their production it seemed clear that their mutual interactions were strong in our technical sense. We have seen that the concept of isotopic spin is the distinguishing feature of strong interactions. It seemed appropriate to assign to these particles isotopic spin values and to demand that in their interactions isotopic spin conservation should hold. This assignment was made on partly theoretical and partly phenomenological grounds by Gell-Mann and Nishijima in 1953. The results were indeed startling. Some of these are:

(1) In any collision process involving π-mesons and nucleons not only are 'strange' particles produced copiously, but there must always be at least two of them. For example, $\pi^- + p \rightarrow \Sigma^- + K^+$. This is called associated production.

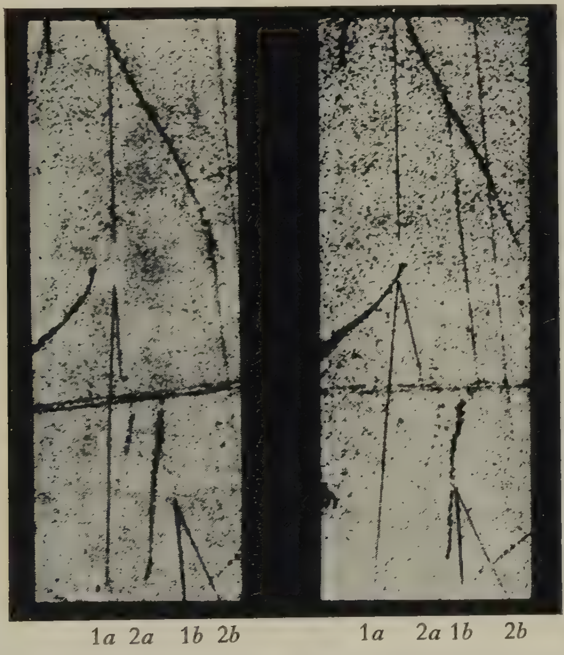

Fig. 7. Stereoscopic pair of bubble-chamber pictures revealing associated production $\pi^- + p \rightarrow K^0 + \Lambda^0$. The *a* tracks are those of the subsequent decay of the Λ^0 giving p (1*a*) and π^- (2*a*); the *b* tracks the decay of the K^0 to π^+ (1*b*) and π^- (2*b*).

(2) In this same collision $\pi^- + p \rightarrow$, it is possible to produce $\Lambda^0 + K^0$, $\Sigma^0 + K^0$, or $\Sigma^- + K^+$ but never $\Sigma^+ + K^-$ even though the total charge, the heavy baryon number etc., all balance on both sides of the reaction. The explanation of this in terms of isotopic-spin conservation law is immediate. We notice from the table that the total I_z value for $\pi^- + p = -1 + \frac{1}{2} = -\frac{1}{2}$. For $\Sigma^- + K^+$, for example, it is indeed $-1 + \frac{1}{2} = -\frac{1}{2}$ but for $\Sigma^+ + K^-$, $I_z = -\frac{3}{2}$ and thus this reaction cannot take place.

A Pythagorean at this stage may well say Number Rules the Universe, and so far as we know he would be right. Once again every one of the predictions made by Gell-Mann's theory has been verified.

Let us now summarize all our findings.

(1) *Strong interactions*

Strong interactions take place between p, n; Λ^0, $\Sigma^{0\pm}$, Ξ^{0-}, $\pi^{0\pm}$, K^{0+}; and their anti-particles. These are characterized by the following set of conservation laws.

(i) Conservation of charge Q.

(ii) Conservation of baryon number

$$N_p + N_n + \ldots - (N_{\bar{p}} + N_{\bar{n}} + \ldots).$$

(iii) Conservation of energy and momentum (space–time translation symmetry).

(iv) Conservation of spin (space–time rotation symmetry).

(v) Conservation of isotopic-spin (isotopic-spin rotation symmetry).

(vi) Particle and anti-particle behave similarly (charge conjugation symmetry).

Subject to these rules any interaction which can take place does take place.

(2) *Electromagnetic interactions*

Medium-strong interactions involve electromagnetic interaction of all charged particles.

These interactions are responsible for π^0 and Σ^0 decay in times shorter than 10^{-17} sec. The conservation laws, except (v), are the same as for strong interactions.

(3) *Weak interactions*

Weak interactions are responsible for the spontaneous decay of all particles except p, e, ν and the photon. The conservation laws are again applicable, except for (v), which is peculiar to the strong interaction.†

9. SYMMETRY IN SPACE AND TIME

We now come to the most recent development in elementary particle physics, which brings us up to January 1957. I have mentioned before the translation symmetry of space–time, and its rotation symmetry. In addition to these symmetries, there are further symmetry properties associated with space–time. These are

(i) space-reflection symmetry, and

(ii) time-reflection symmetry.

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† To be more precise, strong interactions are rotation-invariant for all rotations in the [3] isotopic space, so that not only T_z the component of isotopic spin in one specified direction is conserved but also the total isotopic spin $|\boldsymbol{T}|$ is conserved. Electromagnetic interactions conserve T_z but not $|\boldsymbol{T}|$. Weak interaction conserves neither T_z nor $|\boldsymbol{T}|$.

Gell-Mann has formulated T_z-conservation in a different but completely equivalent way. In so far as charge is always conserved in all interactions, one may consider replacing T_z-conservation by conservation of a quantity S, defined as $S = 2(Q - T_z)$ for π and K-mesons, $S = 2(Q - T_z) - 1$ for heavy particles and $S = 0$ for photons. This quantity has been called ' strangeness ' by Gell-Mann and its values are

$S = +1$ for (K^+K^0) $S = 0$ for (p, n), (π^+, π^0, π^-)

$S = -1$ for (K^0, K^-), $S = -2$ for (Ξ^0, Ξ^-)

Strong and electromagnetic interactions conserve S; in weak interactions S seems to change by one unit.

Consider space-reflection or mirror-reflection first. Since in a mirror a right-hand reflects into a left-hand, space-reflection symmetry is the same as right–left symmetry. The idea that right and left are mutually indiscernible is as old as Leibnitz who first gave it a precise formulation. From Leibnitz's day up to January 1957 we had believed that physical laws do not possess any right or left preference.

Let me state the precise form in which the law of space-reflection had been formulated. Just as Dirac showed that for a particle there must exist an antiparticle, the law of space-reflection asserts that if a particle exists, the one obtained by reflecting it in a mirror must also exist. If a reaction can take place, the corresponding reaction seen in a mirror is also a physically possible one. Thus if right-polarized electrons exist, left-polarized electrons must also do so in nature. If right-polarized neutrinos can exist, so must left-polarized neutrinos. In the biological field we know that most of us have hearts on the left. This is not a negation of the mirror symmetry principle because 'right-hearted' individuals do exist. The fact that there are fewer of them is a consequence of what Hermann Weyl calls contingency—a consequence of the asymmetric initial conditions rather than due to any asymmetry in fundamental law.

The mirror symmetry principle is philosophically appealing. Even more important than any philosophical argument, the principle is known to hold for all strong and electromagnetic interactions. Thus, for example, in the course of experiments with electrons emitted by heated filaments it had been checked that both left- and right-polarized electrons do exist. In summer 1956 Yang and Lee pointed out that there had been nothing so far to prove or disprove the principle for weak interactions, and they suggested a number of experiments which might clinch the matter. So firm however was the belief that the principle must hold for all interactions that Pauli wrote to Weisskopf on 17 January 1957:—

"I do *not* believe" (and the *not* is heavily underscored by Pauli), "that the Lord is a weak left-hander, and I am ready to bet a very high sum that the experiments will give symmetric results."

The experiments were completed two days after Pauli wrote; they have since been repeated all over the world; they showed unequivocally that in weak interactions there is no right–left symmetry. More precisely the experiments showed that left-polarized neutrinos exist but right-polarized neutrinos do not. Like Hoffman in Offenbach's opera, on reflecting a neutrino in a mirror, one sees nothing. It is sobering to think that the results could have been discovered 10 years back for the evidence existed on all the photographic plates with π–μ–e decay. If reflection symmetry holds, relative to the μ-direction of motion an equal number of electrons should be emitted in the forward and in the backward direction. If anybody had bothered to count the numbers he would have discovered the asymmetry. On 27 January 1957, Pauli writes:

> "Now after the first shock is over, I begin to collect myself. Yes, it was very dramatic. On Monday, the 21st at 8.00 p.m., I was supposed to give a lecture on the neutrino theory. At 5.00 p.m. I received three experimental papers; I am shocked not so much by the fact that the Lord prefers the left-hand but by the fact that he still appears to be left-right symmetric when he expresses himself strongly. In short, the actual problem now seems to be the question why are strong interactions right and left symmetric."

One can perhaps give the deeper reason why right-left symmetry should be violated whenever a neutrino is emitted. It can be shown that a rest-mass of exactly zero for the neutrino is incompatible with right–left symmetry. We have lost the symmetry principle but perhaps gained an exact statement about the neutrino mass. However, for decays not involving neutrinos even this explanation for violation of mirror symmetry is not available.

Eddington once asked the following question: if we have only a radio contact with a distant star, could we convey to the inhabitants of this star which of the two types of screws we have by convention chosen to call the right screw? With the violation of the space-reflection principle we know the answer. All we have to tell our distant friends is that our conventions are such that we call the neutrinos emitted in neutron decay right-polarized.

10. TIME-REFLECTION

The time-reflection principle asserts symmetry between past and future; in our formulation it does not make statements about causation but merely statements like the following:

The number of K's and Σ's produced in $\pi^- + p$ collision is the same as the number of $\pi^- + p$ produced in reverse $K + \Sigma$ collision.

Particle		Rest-mass	Spin	I_z	Decay-time (sec)	Decay products	Anti-particle
1	γ	0	± 1		∞	Stable	γ
2	ν	0	$-\frac{1}{2}$		∞	Stable	$\bar{\nu}$ (spin $+\frac{1}{2}$)
3	e^-	1	$\pm\frac{1}{2}$		∞	Stable	e^+
4	μ^-	207	$\pm\frac{1}{2}$		10^{-6}	$e^- + \nu + \bar{\nu}$	μ^+
5	π^+	273	0	1	10^{-8}	$\mu^+ + \nu$	$\pi^-(I_z = -1)$
6	π^0	264	0	0	10^{-16}	2γ	π^0
7	K^+	966	0	$\frac{1}{2}$	10^{-8}	$\mu^+ + \nu$, 2π, 3π	$K^-(I_z = -\frac{1}{2})$
8	K^0	966	0	$-\frac{1}{2}$	10^{-10}	$\pi^+ + \pi^-$, $\pi^0 + \pi^0$	$K^0(I_z = +\frac{1}{2})$
9	p^+	1836	$\pm\frac{1}{2}$	$\frac{1}{2}$	∞	Stable	$\bar{p}$
10	n^0	1839	$\pm\frac{1}{2}$	$-\frac{1}{2}$	10^3	$p + e^- + \bar{\nu}$	$\bar{n}$
11	Λ^0	2180	$\pm\frac{1}{2}$	0	10^{-10}	$p + \pi^-$, $n + \pi^0$	$\bar{\Lambda}^0$
12	Σ^+	2345	$\pm\frac{1}{2}$	1	10^{-11}	$p + \pi^0$, $n + \pi^+$	$\bar{\Sigma}^+$
13	Σ^0	2331	$\pm\frac{1}{2}$	0	10^{-18}	$\Lambda^0 + \gamma$	$\bar{\Sigma}^0$
14	Σ^-	2331	$\pm\frac{1}{2}$	-1	10^{-10}	$n + \pi^-$	$\bar{\Sigma}^-$
15	Ξ^0	2590	$\pm\frac{1}{2}$	$\frac{1}{2}$	10^{-10}	$\Lambda^0 + \pi^0$	$\bar{\Xi}^0$
16	Ξ^-	2590	$\pm\frac{1}{2}$	$-\frac{1}{2}$	10^{-10}	$\Lambda^0 + \pi^-$	$\bar{\Xi}^-$

Elementary particles (1959)

Again it is known that the principle holds in strong interactions, as well as electromagnetic interactions. It is strongly suspected that the principle holds also for weak interactions. The fact that this symmetry principle seems to be a universal law is welcome indeed for the following reason. One of the rather exotic assertions of current theory of elementary particles is the following:

If it were possible to run the reel of time backwards, the time reflection of a given physical situation would correspond, so far as physical measurements are concerned, to a situation which one would normally see by reflecting the physical situation in a space-mirror except that all particles would be replaced by their anti-particles. Thus the time reflected state corresponding to a left-polarized electron is a right-polarized positron. Time reflection symmetry therefore asserts that if left-polarized neutrinos exist in nature so must also right-polarized antineutrinos. This is just what some recent experiments seem to indicate.

11. NEUTRINO EXPERIMENTS

It is perhaps worth while to state more explicitly the use of the terms ' neutrino ' and ' antineutrino '. In neutron decay, the electron is always found to be left-polarized (e_L). As stated before, this contrasts with the case for electrons emitted by heated filaments (electromagnetically), where both polarizations are found in about equal numbers. The contrast arises from the lack of mirror-symmetry for weak interactions. From angular-momentum conservation it can be inferred that the neutral zero-mass particle accompanying the electron in neutron decay must be right-polarized (ν_R). This object has in recent literature been called the ' antineutrino '.

The prefix ' anti-' was used for ν_R to guarantee the (suspected) law of lepton conservation, which would state that if an electron is a leptonic *particle* the object emitted with it in neutron decay should be a leptonic *anti-particle.* With these assignments one could predict for example that in π^- decay ($\pi^- \rightarrow e + \bar{\nu}$) the neutrinic object emitted should be right-polarized (ν_R). This prediction has not yet been experimentally verified.

If time-reflected symmetry holds (as we suspect it does) for the ' time-reflected ' process of antineutron decay, one could predict $\bar{n} \rightarrow \bar{p} + e_R^+ + \nu_L$; that is, the neutrinic object emitted is left-polarized. Consistently with the rest of the nomenclature one could justifiably call this particle a ' neutrino '.

12. CONCLUSION

Before we conclude, let me make some general remarks. First, the entire development presented here is based on assuming that the structure of space and time is the same as that revealed by the special theory of relativity. There are two reasons why we have ignored the general theory of relativity and with it all mention of the fourth and the most pervading universal force, i.e. the gravitational force. Firstly, the gravitational force is a still weaker force than any that have been considered. It has a strength of 10^{-39} in the units used above, and surprising as it may seem, to an excellent approximation its effects for elementary particle physics can be completely neglected. Second, as Wigner has recently emphasized, we have learnt to reconcile the Special Theory of Relativity and quantum mechanics. So far we have no reconciliation of the General Theory of Relativity with quantum theory. Once this comes about we may get a clue to resolving some of our difficulties.

Second, why are there just these particles? Are there still more particles to be discovered? In the case of particles possessing strong interactions, we believe we understand the deeper reason for their existence in terms of the isotopic-spin space. These particles correspond to the representations of the isotopic rotation group. Why this group exists, we do not know. But granted its existence, it would seem that we may have already discovered all the particles that can take part in strong interactions except possibly for one particle. This is a rash statement, of the kind that has been made again and again in the history of physics and has always proved false, and I must qualify it by saying that the isotopic rotation group may admit other particles, but they will all probably have life-times shorter than 10^{-19} sec. Experimental discovery must wait on techniques available at any given time—of the particular 'windows on nature' which may be open. The Wilson cloud-chamber, the photographic emulsion and the bubble-chamber all fail of their nature to show up directly particles with life-times shorter than 10^{-13} sec. Of course, the story is different if one has reason to suspect the existence of a short-lived particle on theoretical grounds, as was the case for π^0 and Σ^0 particles. One can then arrange experiments in such a way that these particles then 'show up', at least indirectly.

For particles falling in the electromagnetic and weak interaction category we still do not know the deeper symmetry principle associated with them. There may well be, for example, more companion particles with the μ-meson. In fact all we know about the particles that fall in these two categories is that their interactions violate some of the strong-interaction symmetries. These interactions in a sense seem to have a negative role. There must in nature be a hierarchy of symmetry principles, some of which are dearer to Nature's heart than others. But why?

Quantitatively we have yet to explain theoretically the ratio of the masses of the particles and (still more difficult) to explain the actual strength of an interaction—the ratio $1:10^{-2}:10^{-12}$. In familiar language this is the same problem as explaining why the dimensionless constant $e^2/\hbar c$ equals 1/137. Deepest problem of all—among all this wealth of particles what is the criterion of elementarity? Are all these sixteen particles elementary or could some of these be considered compounds made from others?

One of the recent attempts at this problem is that of Heisenberg who found it necessary to carrying through his programme to introduce a new and audacious idea in physics. He postulated that space–time does not constitute a continuous manifold—that there exists a fundamental length much smaller than any lengths we are used to, such that within distances shorter than this length no physical measurements are even conceptually possible.

Heisenberg's attempt has been partially successful; it would be truer to say that we do not at present know of a convincing criterion of elementarity. But the situation can change almost overnight with one penetrating flash of insight just as it did in 1926 with non-relativistic quantum theory. And I am confident it will.

As my final thought I would like to end by quoting from the Holy Book of one of the Great Religions of the world:

Thou seest not in the creation of the All-merciful any imperfection.

Return thy gaze; seest thou any fissure? Then return thy gaze again, and again, and thy gaze comes back to thee dazzled, aweary.

The Author:

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Cyclotron Resonance in Solids

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Summary

A new kind of experiment is described which measures the ' effective mass ' of mobile electrons in semiconductors and metals. Samples are placed in a magnetic field which causes the charge carriers to move in circular paths, and a microwave oscillating electric field accelerates them in the same way as charged particles in a vacuum are accelerated in a cyclotron. The effective mass thus determined measures the momentum transfer between the crystal lattice and the electron waves and thus gives detailed information about the nature of the conduction electron states.

1. THE CYCLOTRON RESONANCE EXPERIMENT

This article will describe the application of some well-known principles of electromagnetic theory to a modern problem of solid state physics. It is an interesting example of the way in which the development of experimental techniques has made a new kind of investigation possible. It has the further interest that although the experiments are essentially classical, the results can only be made reasonable if we take account of the role of quantum mechanics in the description of solids. We shall begin by describing the nature of the problem.

The problem is the behaviour of the mobile charge carriers in solids. In both semiconductors and metals, electrical conductivity occurs as a result of the transport of charge by freely moving electrons. We can describe this classically by picturing the conduction electrons as point charges which are free to move within the solid-like gas ions in a vacuum, under the influence of an externally applied electric field. It is clear, however, that this can be at best only a qualitatively correct description, because it is known that in such a situation as exists inside a solid the electrons must be described in terms of waves which interact with the periodic lattice of the solid. In fact it has long been realized that even in an ideally perfect crystal lattice (in which there would be no collisions between the charge carriers and atoms of the crystal), acceleration of charge carriers by an external field would cause a continuous transfer of momentum between the charge carriers and the crystal lattice. This continuous momentum transfer can seriously alter the behaviour of the mobile carriers when an external field is applied. This seemingly peculiar effect can be made to seem more natural if we recall the way in which x-rays are diffracted in a crystal. In the early days of x-ray investigations it was found that although in general x-rays are easily transmitted through crystals, for particular wavelengths of x-rays, interference effects between the x-ray waves and the periodic crystal lattice cause strong reflection of the x-rays. This is the so-called Bragg reflection, which has given physicists so much insight into the wave nature of x-rays, and has provided detailed knowledge of the geometric arrangement of atoms in crystals. The connection of this work with our problem of mobile electrons in crystals results from the fact that the electron is describable by a group of waves very similar to x-ray waves. The spectrum of the wavelengths will depend on the momentum of the electron, so that when the electron is acted upon by an external field, its momentum and therefore its wavelength spectrum is modified. Furthermore, since some wavelengths will be strongly reflected by Bragg-like interference effects, there can be large momentum exchange between wave components of the electron and the crystal lattice. This momentum transfer is usually taken into account by ascribing a false value to the conduction electron mass, called the effective mass, m^*. Thus if an external applied field increases the Bragg-like reflection of electron wave components, resulting in loss of momentum to the lattice, the effective mass is greater than the true electronic mass. In other situations the external field may cause the crystal to transfer momentum in the forward direction to the electron (by reducing the Bragg-like reflections), resulting in greater acceleration than that to be expected from the external field alone. This gives an effective mass less than the real electronic mass. We might describe the effect by writing $F+F'=ma$, where $F=eE$ is the force of the external field E on the electronic charge e, and F' is the effective force on the electron due to momentum transfer from the lattice. m is the electronic mass and a is the resultant acceleration. In terms of the effective-mass concept we would replace the above equation by $F=m^*a$ where m^* is modified so as to give the same acceleration as is obtained from the earlier equations. Note that since the Bragg-like reflection depends on the direction of travel of the electrons through the crystal (as with x-rays), the effective mass will in general vary with the orientation of the crystal with respect to the applied field.

This seemingly strange behaviour of conduction electrons is the chief concern of the experiments to be discussed. Prior to these studies there were a number of experimental results which in rather indirect ways gave indications that the theory of the effective mass rather crudely described above was correct. But very much in the minds of quite a number of solid state physicists was an experiment which would give a direct measure of the effective mass. Although there were originally many frustrating practical difficulties, the idea of the experiment is so simple it is not surprising that it was originally thought of by a number of physicists almost simultaneously. For free particles in a vacuum the idea goes back to Drude, Voigt and Lorentz. The first published suggestions that it might be possible to perform a direct experiment in solids were given by J. Dorfmann in Leningrad and by R. B. Dingle in Cambridge in 1951, and by W. Shockley in the United States in 1953, who applied the ideas in a form suitable for the semiconductors silicon and germanium. The experiment is called the cyclotron resonance experiment because of its obvious relationship to the principle of the cyclotron.

The idea of the experiment is based on the motion of a charged particle in a uniform magnetic field. According to classical theory the component of motion of a charged particle of charge e and mass m perpendicular to a magnetic induction field B is circular and has a rotational frequency f cycles per second of

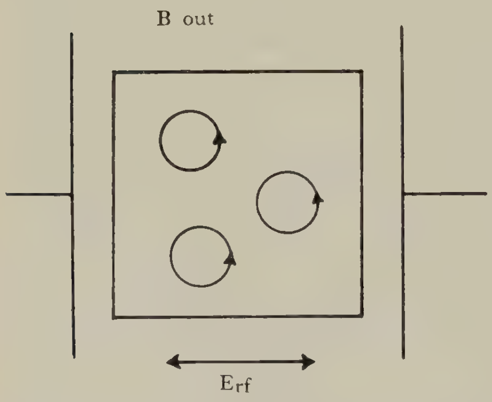
$$f=\frac{Be}{2\pi m}$$

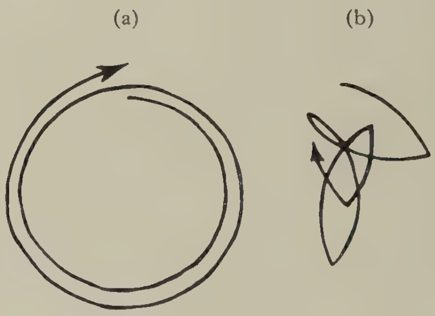
This so-called cyclotron equation is readily understood if we apply Newton's laws of motion to a charged particle moving under the influence of the magnetic field. If we consider only that component of motion perpendicular to the field we may write

$$F = ma = mv^2/r$$

where v^2/r is the centripetal acceleration which must be provided by the magnetic force $F = Bev$, where the velocity v is perpendicular to B. If we substitute $v = 2\pi rf$ for the velocity in terms of the frequency of rotation and the radius of the circular path, we get the equation given above for the 'cyclotron frequency'. A special feature of this well-known equation is that the frequency of rotation is independent of the radius of the circular path and also of the velocity. This fact allows particles of a given charge-to-mass ratio to be accelerated in a cyclotron by the application of a radiofrequency electric field having a constant frequency. In a cyclotron this electric field results from the oscillatory voltage applied between two half-circular hollow electrodes which surround the path of the charged particles. Each time the particles pass through the gap between electrodes the timing is such that they are accelerated by a force Ee. The result is a spiralling outward of the particles as they gain kinetic energy, until they finally emerge from the perpendicular magnetic field as high energy particles.

Let us now see how the cyclotron principle can be applied to measure the effective mass of mobile charge carriers in a semiconductor. It will turn out that in the practical cases, very high frequency electric fields must be used, involving the use of microwave techniques, but we will first describe the experiments as though they were performed in the more usual lower frequency range in order to clarify the principles involved. A single crystal sample of the material to be tested is placed in a uniform magnetic field which will cause any moving charges to move in circles in a plane perpendicular to the field. (They may also have velocity components parallel to the field, which may be neglected since the magnetic force will not act on these components.) The problem now is to measure the frequency of revolution of the moving charges, since this will allow us to evaluate m^* from the cyclotron equation, where we replace m in the equation by m^*, the effective mass. Figure 1 illustrates an arrangement which would in principle make this measurement possible. It shows the orbits of a few charge carriers in the crystal, and also indicates two parallel electrodes which, when connected to an electrical oscillator, will produce an alternating electric field E in the sample. If the frequency of the oscillating electric field is arbitrary, nothing in particular will happen. A charge carrier while it is moving in the direction parallel to the force eE of the electric field will be accelerated. But a short time later the oscillating field will have reversed and the carrier will be reduced in velocity. On the average there will be no net effect on the carrier motion. However, suppose the frequency of the alternating field is adjusted to the exact value given by the cyclotron equation when the proper values of e, B and m^* are inserted. In these circumstances the rotational frequency of the carriers will be just the same as the oscillating frequency of the electric field. There will be some carriers which while travelling to the right in their orbit will be accelerated by the electric field, and while travelling to the left will continue to be accelerated by the field which will have reversed direction in synchronism with the carrier motion. Such carriers will gain energy continuously from the electric field, and their increasing velocity will cause them to spiral outwards into larger and larger orbits. We would also expect an equal number of carriers to be in the opposite phase, that is, to move in their orbits so that the force of the field acted continuously to slow them down and lower their velocity and decrease the radii of their orbits. On first thought we might expect that these two effects would cancel, resulting in no net change in the total kinetic energy of the carriers. Closer examination shows that there is a net increase in the total kinetic energy of the carrier population. Let us simplify the problem by assuming two equal groups of carriers, one exactly in phase with the electric field and one exactly out of phase with it. Let the original velocity of all carriers be v and let the change due to the action of the electric field be $\pm\Delta v$. After a certain number of revolutions the two groups will have velocities of $v+\Delta v$ and $v-\Delta v$. Since the kinetic energy is proportional to the square of the velocity, the net change in energy is proportional to $(v+\Delta v)^2-(v-\Delta v)^2=4v\Delta v$. Thus there is a finite change in the total energy of the electrons. We now have the very important fact that when the frequency of the electric field is exactly equal to the rotational carrier frequency, energy is absorbed by the carriers, and this energy must come from the source of the alternating electric field. It is this effect which can be used to measure the effective mass. If we had some method of measuring the energy absorbed from the electric oscillator we could set the oscillator frequency at some convenient value and then vary the magnitude of the external magnetic induction field B. When the resonance condition as given by the cyclotron equation was satisfied there would be a sharp increase in the energy loss from the electric oscillator. From the value of B required for resonance the effective mass could be calculated from the equation.





(Left) Fig. 1. Schematic diagram of cyclotron resonance arrangement. A semiconductor crystal is placed in a magnetic field B and in an oscillating electric field E_{rf}. Resonance absorption occurs when the electric field is synchronous with the circular motion of mobile charges, causing them to gain energy from the electric field. In an actual experiment the electric field is produced in a microwave cavity.

(Right) Fig. 2 (*a*). Spiral orbit of an electron in cyclotron resonance under ideal condition of no collisions; (*b*) Orbit in which there are a number of collisions during an r.f. cycle. No resonance absorption is possible under such conditions.

The description we have given of cyclotron resonance absorption of energy is correct in principle. But we have tacitly assumed a very simplified and unrealistic model for the crystal being tested, and have so far not mentioned the more complicated situation in a real crystal which seriously modifies the motion of carriers as described above. Only for an ideally perfect crystal lattice is the motion of the carriers dependent solely on the Bragg-like interference of electron waves with the lattice described by an effective mass. The lattice of a real crystal is not perfectly periodic but has various kinds of irregularities in the spacing between atoms. The effect of these irregularities is complicated, but can be described crudely as causing a classical scattering of the electrons by the imperfections. It is useful to characterize this effect of imperfections by speaking of the mean free path of the charge carriers in the crystal, just as we use the mean free path to describe the average distance between collisions of molecules in a gas. As a matter of fact, the mean free path for mobile carriers in solids under normal conditions is disappointingly small. This has a very serious effect on the cyclotron resonance experiment which we have described. In fig. 2 (*a*) we show the situation in the ideal crystal when the cyclotron resonance condition is satisfied, showing the increasing radius of an electron orbit as the electron absorbs energy from the synchronous electric field. Contrast this with the usual situation for a carrier in a real crystal, as shown in fig. 2 (*b*), where the effect of collisions is shown. In this latter situation it is clear that there can be no resonant absorption of energy from the electric field, since there will be no condition under which the electric field can remain in phase with the carrier motion when it is being scattered by such frequent collisions. This difficulty is indeed the central problem which must be solved in order to allow the experiment to work. In the next section we will explain the various steps which were taken to improve the situation and which led to the present success of the experiments.

2. IMPROVEMENTS IN THE MEAN FREE PATH

Let us first examine in more detail the important kinds of imperfections which can limit the electron mean free path in a crystal. We will mention first the presence of foreign atoms in the crystal. Foreign atoms, or impurities, will act as scattering centres, even if they are at the site of an atom of the parent crystal, since the electric field around the impurity atom will not be identical with that of the atom it replaces and so there will be a perturbation of the otherwise periodic arrangement of identical atoms. The effect will be more serious if the effective charge on the impurity atom is different from that of the crystal atoms since the field perturbation will be greater. If the foreign atom takes an interstitial position between normal atom sites the scattering from it will be greater than if it is at a normal atom site. A similar source of trouble will be any distortion of the lattice due to lattice vacancies or other departures from regular spacing of the normal crystal atoms. This latter effect turns out not to be of major importance in most cases. Another effect, which can be the dominant one, is the effect of thermal vibrations. Even if we had an otherwise perfect crystal, thermal vibrations at room temperature would produce large enough departures from regular lattice spacing to limit seriously the electron mean free paths.

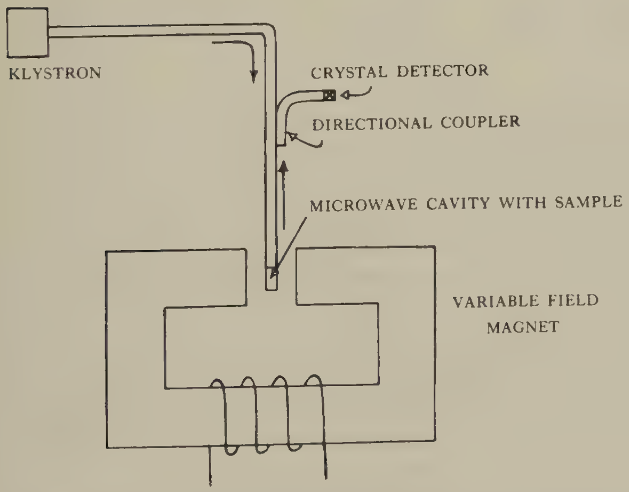
Now we will discuss the various steps which can be taken to minimize the scattering and increase the mean free paths. An obvious first step is to obtain crystals of the highest possible purity. It is no accident that the first successful experiments were performed on silicon and germanium after the greatly improved techniques for purification of these substances were developed under the impetus given by the invention of the transistor. This is a very good example of the impact of improvement in technology on the advance of pure science. The requirement of pure materials for the rapidly developing transistor industry forced a tremendously rapid improvement on the zone refining method of purification, which we will not have space to discuss here, to such a point that in 1953 single crystals of both silicon and germanium were available which had less than one atom of impurity per 10^{12} atoms of the crystal. Historically, this was the final development necessary for the successful experiment. An equally important requirement, but one easily achieved at a much earlier date, was the cooling down of the sample to be tested to a very low temperature. This was achieved by the use of well-known low temperature techniques, using liquid helium at 4·2°K. At this temperature thermal vibrations are completely unimportant as a source of crystal imperfections, and full advantage can be taken of the long mean free paths allowed by the high purity of the material.

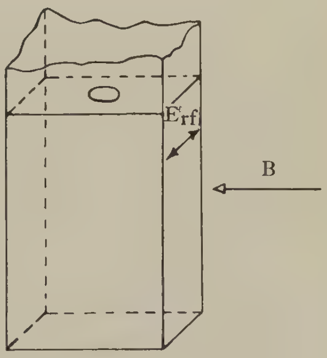
Once a maximum practicable mean free path had been obtained by the use of high purity materials at low temperatures there was only one other way in which the likelihood of a successful experiment could be increased. This was by increasing the frequency of the oscillating electric field as far as possible. It turns out that the important criterion for the successful observation of cyclotron resonance is that, roughly speaking, between collisions the carriers make at least one revolution in their orbits in the applied magnetic field. Clearly, the higher the frequency of oscillation, the higher will be the magnetic field which matches this frequency, according to the cyclotron equation, and the smaller will be the circular orbits of the carriers. The chance of the carriers completing one revolution will, of course, increase as the size of the orbit decreases. Fortunately, the modern development of microwave techniques provided high frequency sources (as well as the equipment for using them) which were well into the range which made the experiment likely to succeed. Successful experiments have involved the use of frequencies of 10 000 megacycles/second and higher. As a matter of fact it turned out that the apparatus which had been developed in both England and the United States for the observation of paramagnetic resonance in solids could be used with only slight modification for the observation of cyclotron resonance.

3. The experimental method

We may now turn to the experimental arrangement used to observe cyclotron resonance, and shall give a brief qualitative description of the method used. Figure 3 shows a simplified diagram of the circuit. For the sake of clarity we have omitted in the diagram the low temperature apparatus which surrounds the sample as well as the complicated electronic circuits which stabilize the microwave frequency, and the magnetic field. We also omit the electronic circuits which process the signal received in order to minimize extraneous signals which could obscure the cyclotron resonance signal.

The alternating electric field which is used to accelerate the carriers in their circular paths in the magnetic field comes from the microwave electromagnetic field which circulates in the hollow metal cavity placed in the gap of the electromagnet. The source of microwave power is a klystron tube. The microwaves are conducted through a hollow metallic waveguide through a small coupling hole in a metal diaphragm which separates the guide from a so-called resonant cavity. At microwave frequencies this cavity plays the role of an *LCR* resonant circuit at lower frequencies. That is, for a particular frequency, the electromagnetic waves incident on it through the coupling hole circulate within it with very small losses. Most of the energy is in the form of standing electromagnetic waves, and what losses there are result from circulating currents induced in the inner surface of the metal walls by the standing waves. Unlike the lower frequency counterpart, involving lumped capacity and inductance, such a cavity has many resonant frequencies. However, a geometry and frequency is chosen such that only one mode of oscillation is excited and the cavity is oriented so that the alternating electric field is in one direction, in a plane perpendicular to the applied magnetic field. Figure 4 shows a sketch of a rectangular resonant cavity, connected to the rectangular waveguide by a coupling hole in the wall of the cavity. The electric field vector is perpendicular to the applied magnetic field as shown, and the alternating magnetic field vector which does not directly concern us in this experiment, makes loops in a plane perpendicular to the electric field. A sample placed inside the cavity is thus in a situation exactly like that shown in fig. 1. The necessary experimental procedure is clear :





(Left) Fig. 3. Microwave apparatus used in cyclotron resonance experiment. Microwaves generated by the klystron travel through the hollow waveguide to the sample in the cavity. When the magnetic field is adjusted to the resonance condition, changes in microwave losses result in changes in the power reflected back to the crystal detector.

(Right) Fig. 4. Rectangular resonant cavity, showing coupling hole to waveguide. The sample to be tested is placed inside this cavity.

holding the microwave frequency constant, at the value which excites the desired mode of electromagnetic oscillation in the cavity, it is only necessary to sweep the external magnetic field through all values, and look for the particular value for which the cyclotron equation is satisfied. For this particular value of magnetic field there will be a peak in the power dissipation within the cavity, due to the extra energy used in accelerating the carriers in the sample. If we have some way to measure the losses within the cavity we can find this peak in the absorption and can proceed to measure effective masses as required. As a matter of fact, just such a method is available to us. In one form it simply involves the measurement of the power reflected from the cavity, which varies when losses within the cavity change. This can be readily understood if we go to the lower frequency analogue of the microwave circuit, as shown in fig. 5. We have drawn an *LCR* resonant circuit coupled by mutual inductance to a transmission line. The mutual inductance takes the place of the coupling hole which couples the microwave cavity to the waveguide in the microwave system. It is clear that in general there will be some power reflected back into the transmission line through the mutual inductance when the frequency used excites the resonant mode of the *LCR* circuit. Furthermore, any change in the resistance shown will certainly modify the amount of reflected power, since it will affect the current flow in the circuit. This would appear to be exactly equivalent to a variation of losses within the microwave cavity. This qualitative reasoning is correct, and leaves us with only the problem of measuring the microwave power reflected by the cavity. Figure 3 shows one method of making this measurement. Some of the power reflected by the cavity is picked up by the directional coupler and transmitted to a crystal detector. The directional coupler is a sort of directional antenna which picks up microwave power flowing in only one direction. Interference effects cancel out the signal from power flow in the opposite direction. The detector crystal rectifies the microwave signal and converts it into a d.c. current which can be measured by conventional current meters. It is the variation of this current with magnetic field which allows the determination of the resonance conditions.

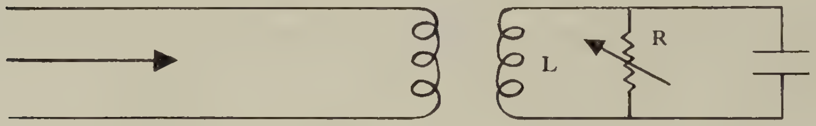


Fig. 5. Lower frequency analogue of microwave cavity. The LCR circuit replaces the cavity and is inductively coupled to the transmission line. Changes in microwave losses in the cavity correspond to change of resistance in the circuit.

It was stated earlier that this experimental apparatus is very similar in principle to the apparatus used in the study of paramagnetic resonance in solids. The only important difference is in the orientation of the cavity in the magnetic field. In the cyclotron resonance experiment it is the high frequency electric field vector which acts on the electrons, and this field must be perpendicular to the applied magnetic field. In paramagnetic resonance it is the high frequency magnetic field which acts on the magnetic dipoles of the electrons, and so it is the high frequency magnetic field which must be perpendicular to the applied magnetic field. In the usual case, all that is required to switch from one kind of measurement to the other is to rotate the microwave cavity through 90°.

4. PROPERTIES OF A SEMICONDUCTOR

Before describing the results of the cyclotron resonance measurements we must discuss some relevant properties of semiconductors. An important property of all pure semiconductors is the fact that in the ground state (at absolute zero temperature) all of the electrons fill completely a set of energy levels up to a certain maximum energy level at the bottom of a so-called forbidden energy gap. The energy diagram of fig. 6 shows schematically the allowed energy levels for electrons. The lower region, called the valence band, consists of a very large number of levels which in a pure semiconductor at low temperature are just completely filled with electrons. The Pauli exclusion principle dictates that no level can be occupied by more than one electron. (Actually, two electrons of opposite spin are allowed in each state, but we shall count these as occupying two states.) Above the valence band is an energy region in which there are no allowed levels for electrons. This is called the forbidden energy gap. Above this is another set of closely spaced levels which at low enough temperatures are completely empty.

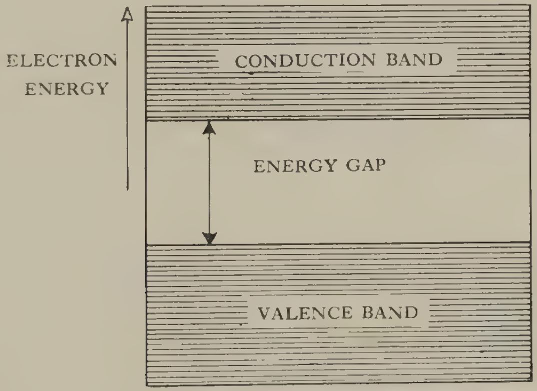


Fig. 6. Semiconductor energy diagram, showing valence band (normally filled), and conduction band (normally empty).

With this model, it is easy to explain why pure semiconductors act as insulators at low temperatures—an effect which is at first surprising, since the diagram implies correctly that the wave functions which describe the electrons in the valence band extend throughout the material. (It is this extension of the electron wave functions throughout the material which justifies the use of the exclusion principle.) On the other hand if we ask how the distribution of electrons will be affected by the application of an external electric field we can see at once that there will be no effect at all. If an electron is to be accelerated by such a field it will have to move to a new level appropriate to its change of momentum. But since all levels are full, there are no new levels available. Thus though the electrons are not localized they cannot respond to an external field, so we have an insulator. However, if by some means a large enough increment of energy can be given to an electron near the top of the valence band to lift it across the energy gap and into the conduction band, this electron will find itself in a region of closely spaced unoccupied levels which will allow it to be accelerated by an external field. Such electrons can thus act to produce conductivity as long as they stay in the conduction band. In addition, each electron which is boosted into the conduction band leaves an empty level in the valence band. Additional conduction can now take place in the valence band by the successive displacement of electrons into the unoccupied states. In effect the unoccupied state can move from one level to another and we can think of the conduction taking place by means of the ' holes ' in the otherwise fully occupied states. The hole moves under external forces as though it had a positive charge.

Although it is not appropriate to justify this model of a semiconductor here, it is worth while to point out that the existence of the forbidden energy gap is the direct result of the Bragg-like reflection of electron waves by the periodic lattice of the crystal. In a rather crude way we can describe the effect as follows : imagine that we attempt to increase the momentum of an electron at the top of the valence band by application of an external electric field. This would change the wavelength of the electron wave just sufficiently for the wave to be reflected by the Bragg-like interference in the crystal lattice. In effect the momentum of the electron would be reversed rather than moved into the region of the energy gap. This prohibition extends throughout the forbidden gap.

On the basis of this very incomplete description of the behaviour of electrons in a semiconductor we may see how to proceed with the cyclotron resonance experiment. If we are to measure the effective mass of conduction electrons, the first problem is to ensure that we have a population of electrons in the conduction band. The most obvious way is to increase the temperature of the material until thermal excitation is able to raise the energy of some of the electrons in the valence band over the forbidden gap and into the conduction band. But this we must not do, since, as we have seen, low temperatures must be maintained in order to allow long mean free paths. A simple solution to this problem is to illuminate the sample with radiation of high enough energy so that the absorption of a quantum of light energy will give an electron enough energy to lift it into the conduction band. For both silicon and germanium, visible light is satisfactory and this method is the one which has for the most part been employed.

5. Experimental results

We have now seen how the experimental situation stood in 1953. Purification methods had been developed which, combined with well-known low temperature techniques and slight modifications of quite familiar microwave methods, made the experiments seem feasible at least on the basis of estimates of the mean free path in the purest crystal of germanium and silicon. The first successful experiments on germanium were reported from the University of California, followed very shortly by similar results from the Lincoln Laboratory of M.I.T. Before long, complete results on both silicon and germanium became available from both of these laboratories, and since then the effective masses in several other semiconductors have been determined.

No attempt will be made here to give a detailed report of the results. Instead we shall use the results in germanium as an illustration of the kind of information which has been obtained. Figure 7 shows the result of a measurement of cyclotron resonance absorption in a germanium single crystal in one orientation. A number of absorption peaks were found as the external magnetic field was varied from zero up to its maximum value. In general, the effective mass is different for each direction of motion of the carrier in the crystal. The peaks shown in the figure each correspond to an average value of the effective mass of a carrier as it performs its circular motion around the magnetic field direction. The value of the effective mass belonging to each peak is obtained by solving the cyclotron equation for each peak, putting in the microwave frequency used and the measured value of the magnetic field. A whole series of experiments using different crystal orientations was required in order to obtain a complete knowledge of the effective mass for all directions of carrier motion within the crystal. There are two particularly interesting features of the results shown, both of which are related to the fact that there are a number of resonance peaks found. The first is that there are several peaks corresponding to the motion of the electrons. More than one electron peak is observed because of the variation of effective mass with orientation of the crystal. The variation has symmetry about the [111] axis, of which there are four in the crystal. With respect to an arbitrary direction in the crystal, each of these axes has a different aspect, so as many as four peaks may be seen corresponding to the different aspects of these axes. The other interesting fact is that some of the peaks are describable in terms of the motion of holes in the valence band. As in the case of conduction electrons, there are two kinds of hole states in the crystal. To the author, it has

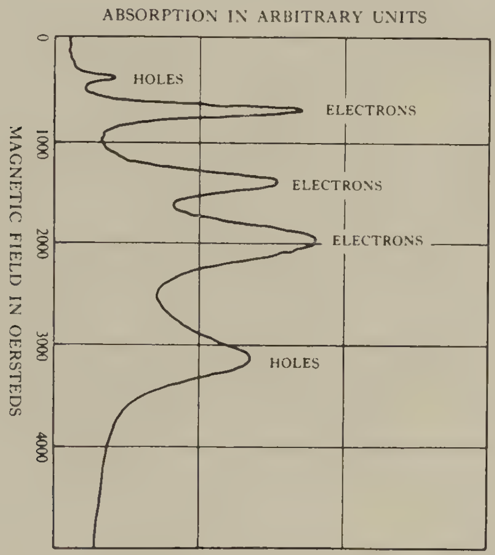


Fig. 7. Cyclotron resonance absorption in a germanium single crystal. Peaks in the absorption correspond to electrons and holes moving around different parts of the constant energy surfaces.

always seemed most remarkable that the peaks ascribed to hole resonance result from the effective circular motion in the magnetic field of empty states in the valence band as one electron after another moves so as to vacate consecutive states. We shall describe below the method by which resonance peaks due to holes can be distinguished from resonance peaks due to electrons. The experimental results on holes provide a striking confirmation of the validity of the concept of the conduction of charge by mobile holes.

The numerical results for the effective mass of electrons in germanium show that the conduction electron state has an effective mass which varies from $0{\cdot}36m_0$ to $0{\cdot}08m_0$, where m_0 is the normal value for a free electron. There are two types of hole states, one having a mass of approximately $0{\cdot}04m_0$ which varies only slightly with orientation, and another which varies between $0{\cdot}28m_0$ and $0{\cdot}37m_0$.

Of perhaps more interest than the exact numerical values of the effective masses is the nature of the electronic states which are indicated by the symmetry of the results. In principle, we should like to be able to start with the complete description of the electronic structure of the atoms making up the crystal and on the basis of the interactions between the atoms when they are arranged to form the crystal structure, to calculate the way in which the conducting states are derived from the original atomic electronic states. Needless to say, such calculations are tremendously complicated and indeed essentially beyond present mathematical methods. However, there are a number of approximate methods, some of which give results which may be at least partially correct. The importance of the experimental results is that they give very detailed descriptions of the conduction states and so serve to show to what extent the approximations which must be made in the calculations are valid. A knowledge of the effective mass is, of course, also of great value in the interpretation of all transport phenomena involving the motion of charge carriers within a solid. A detailed account of the results in silicon and germanium which also more fully describes the experimental method is available in a paper by Dresselhaus, Kip and Kittel (1955.)

6. ELECTRONS AND HOLES

The problem of distinguishing between the resonances due to holes and those due to electrons is made quite simple by the fact that holes move in a direction opposite to the flow of electrons, and therefore behave as though they had a positive electronic charge. The experimental method by which the two kinds of motion have been distinguished has been to replace the effectively plane polarized microwave electric field by a circularly polarized field. Such a circularly polarized electric field can accelerate only those charges which rotate in the same sense as the electric field vector. Since positive and negative charges curve in opposite directions in a magnetic field it is possible to accelerate either one or the other type of carrier separately.

7. CYCLOTRON RESONANCE IN METALS

Our discussion so far has been concerned with the problem of cyclotron resonance in semiconductors only. A problem of at least as much interest is that of measuring effective masses of conduction electrons in metals. Here the experimental situation is quite different, and in fact it at first seemed that there were basic difficulties which would preclude the application to metals of the method used in semiconductors. The difficulty lies in the very high density of carriers in metals. In a normal metal there is one or more conduction electron contributed by each atom, giving a conduction electron density of 10^{21} per cm^3 or more. In contrast, both theory and experiments in semiconductors indicated that at conduction electron densities above, say, 10^{10} per cm^3 the cyclotron resonance experiment is no longer feasible. The reason for this lies in the effect of the high carrier density in polarizing the crystal which is being investigated. Thus if conditions are such that cyclotron resonance is approached, the electrons tend to move synchronously in their cyclotron orbits and there is an alternate displacement of the entire group of electrons first towards one side and then towards the other side of the crystal. For high charge densities this results in a large effective polarization of the sample which adds another electric field to the forces acting on the electrons. The cyclotron equation is then no longer correct since it does not take account of this extra force. The resonance condition is seriously modified and absorption peaks if they are observable at all are shifted and no longer can be used in a simple way to obtain effective masses.

In 1956, however, Azbel and Kaner in Kharkov in their theoretical studies of the surface impedance of metals suggested that meaningful cyclotron resonance experiments might be performed in metals under the special condition that the mean free paths of the conduction electrons were much greater than the so-called skin depth of the metal. The skin depth is a measure of the penetration of a high frequency electromagnetic field into a metal, in which eddy currents tend to cancel the field and cause it to decrease exponentially as it penetrates below the metal surface. For pure metals at low temperatures, the skin depth at microwave frequencies is of the order of 10^{-5} cm. In fig. 8 we show the geometric

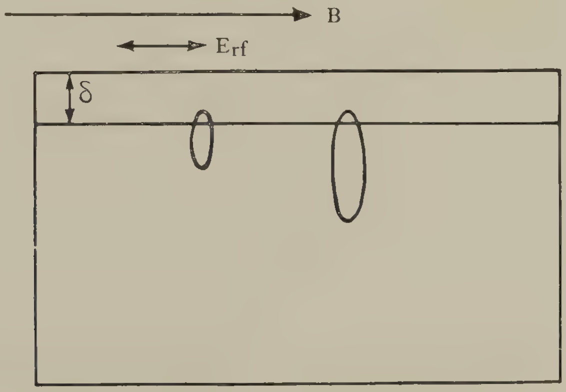


Fig. 8. Cyclotron resonance in a metal. Magnetic and r.f. electric field are both in the surface plane of the metal. The effective electrons are those which emerge into the skin depth region of thickness δ while rotating around the magnetic field direction. Synchronous electrons are accelerated to left or right each time they emerge into the skin depth. Resonance occurs not only for B_0 such that electrons are synchronous with the microwave electric field, but for fields of $B_0/2$, $B_0/3$, etc.

arrangement required. The magnetic field is placed parallel to the metal surface and the high frequency electric field is parallel to the magnetic field. (Another possible arrangement which is quite similar moves the electric field through 90° so that it is parallel to the metal surface, but perpendicular to the applied magnetic field.) As a rough approximation we may think of the electric field as remaining constant within the skin depth and going to zero below it. With this picture it is easy to see how the experiment should work. Let us take an electron whose orbit in the applied magnetic field happens to take it from the interior of the metal up into the skin depth once each revolution. If conditions are such that the cyclotron equation is satisfied, some of these electrons will appear in the skin depth in synchronism with the high frequency electric field so that a force tends to accelerate them to the right. Others, just out of phase with these, will be accelerated to the left. In both cases the electrons absorb energy from the field. Furthermore, there is no longer any polarization effect since electrons are being accelerated in both directions. The absorption peak from these electrons is thus expected to be very similar to those seen in semiconductors, and the same equation may be used. There is, however, another possibility which did not occur in the semiconductor case. Suppose we reduce the applied magnetic field from its value B_0 which satisfied the cyclotron equation for the particular effective mass involved to a value just $\frac{1}{2}B_0$. It will still be true that the electric field will be a maximum each time the electron enters the skin depth, so there should be another absorption peak for the value $B=\frac{1}{2}B_0$. Similarly for values of $B=\frac{1}{3}B_0$, $\frac{1}{4}B_0$ and so forth. Thus we expect a whole series of peaks, corresponding to subharmonics of the motion which occurs when $B=B_0$.

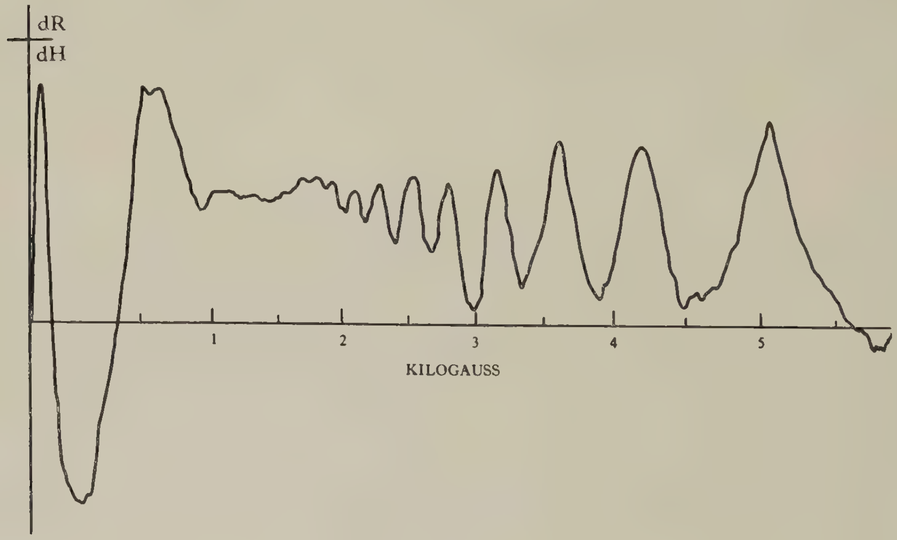


Fig. 9. Cyclotron resonance lines in a single tin crystal. The peak at approximately 5 kilogauss in the curve of the rate of change of resistance with the magnetic field is the fifth subharmonic of the cyclotron resonance. Subharmonics up to the thirteenth are visible. Cyclotron 'masses' from 0·2 to 3 electron masses have been seen in tin. The experiments have been conducted at 4·2°K at a microwave frequency of 24 kilomegacycles/sec.

The first successful observation of such a series of lines was made at the University of California at Berkeley, using a single crystal of tin. In fig. 9 we give a curve obtained which shows up to the twelfth subharmonic. For this highest subharmonic, the electrons are making one complete orbit every 12 cycles of the microwave field. Successful experiments have also been performed on a number of other metals, including copper, aluminium and zinc. At the present time studies of metals by this new technique are in only the early stages, but it is already clear that work now going on in the United States and in England will soon add greatly to our understanding of the behaviour of conduction electrons in metals.

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The Author:

Professor Arthur F. Kip, PH.D., is Professor of Physics at the University of California, Berkeley, California. A graduate of the University of California, he served on Operations Research with the U.S. Navy (1941–45), and was then Assistant Professor of Physics at Massachusetts Institute of Technology from 1945 to 1951. He has specialized in the applications of microwave techniques to solid state physics, in particular in electron spin resonance and cyclotron resonance.

X-ray Crystallography—a Meeting Place of the Sciences

by H. LIPSON
University of Manchester

Summary

In this article I have tried to give an account of x-ray crystallography as a new subject which is still rapidly expanding. Without any doubt it is an important subject because it is the main method for examining the solid state on an atomic scale. It is an absorbing subject because the fundamental problem of interpreting the experimental evidence still exists and therefore intellectual effort must be put into understanding the scientific principles as well as in deriving results. This effort is at present mainly mathematical, and many people believe that it is probable that the ultimate solution must come through mathematics. But in this article I have endeavoured to show that the subject is essentially a physical one, and have described the physical procedures that I hope will reach the winning post first!

This interesting race, and the collaboration of so many other branches of science, make x-ray crystallography a most lively subject to work in.

1. The subdivision of science

The era of the giant in science is long past. The task of keeping abreast of one's own specialism is formidable enough for most scientists, and the emergence of another Newton, Rayleigh or Clerk Maxwell is extremely unlikely. Admittedly one still meets occasional scientists who seem to have unerring instinct for recognizing the vital points of any subject they meet, but they are rare. Most of us are considerably limited, and once we have chosen to specialize we cannot easily turn to another discipline.

For this reason, divisions of science—such as physics, chemistry, botany—have appeared and have become vehicles for education. Most students must definitely decide, before leaving school, which vehicle they are to mount, although some courses, such as the Natural Sciences Tripos at Cambridge, allow the choice to be made at the University. Nevertheless, such courses are becoming rarer, and will probably give way completely to specialist courses in the future.

We must, I suppose, accept these facts. If University teaching is to retain its flavour, there must be opportunity for students to be inspired by lecturers who are at the same time working at the extremes of their subject. Few students can be taken to these extremes in a number of disciplines at once, and it is for this reason that the number has been gradually reduced over the years.

Nevertheless, we must bear in mind the disadvantages that result. The system can breed people with narrow views, who can see only one aspect of a subject, who are even contemptuous of disciplines other than their own. It is important therefore that, in being taught a subject, students should be made aware of the impact of that subject on other branches of knowledge. Physicists, for example, should be aware of the basic facts of chemistry in order that they can properly appreciate modern theories of the solid state. Similarly chemists should have enough physics to understand the principles of so many of the instruments that they use. Both should know something of the impact of their subjects on biology, if only in order that they may appreciate the wonderful design and economy of even the simplest living organism—but also perhaps to assist in solving the basic question in science, ' What is the nature of life? '. In other words, we must look upon the sub-divisions of science as unnatural barriers, forced upon us by circumstance, which should have as many gates and openings in them as possible.

It is worth while asking whether these sub-divisions should remain. Probably they should. Although they have been arrived at by no logical process, it is fair to assume that there is some measure of worth in a subject, such as physics, which has evolved naturally—a kind of natural selection, as it were. (It is possible, on the other hand, that the Universities should consider, more seriously than they have done in the past, combined courses of various kinds. I mean properly designed courses, administered by a professor, not courses consisting of selected lectures and classes that are already in existence.) There is no doubt that many students, even at school, are attracted to a specific subject such as physics ; some are attracted by its development from a few general principles, some by the opportunity of correlating experiment and theory ; and some by the central position that physics holds with respect to other branches of knowledge. They know at an early age that they want to be physicists and they should be catered for.

Nevertheless, we must be aware of the dangers of such early specialization, particularly for the élite ; such students should be obliged to cover a range of study, some of it deliberately unrelated to physics. We do not want to breed the intellectually arrogant, unaware of their own ignorance of large tracts of modern knowledge. I am not, of course, advocating wide ranges of lectures for the physicist ; I do not believe in lectures as a cure for narrow-mindedness! But a few seeds, well planted in a lively mind, will be bound ultimately to produce fine fruit.

In fact, I believe that the best education for a physicist is that which will bring him automatically in contact with other subjects—scientific or otherwise. If he is keen and if he is made aware of the problems that exist, he will *want* to see how his physics bears upon other branches of science ; he will think about the application of physical methods to other human activities such as the arts and politics. He will almost certainly go through the stage when he believes that these methods can be applied with success to such activities, and the gradual discovery that human beings do not behave with the same regularity as atoms will ultimately leave him a fuller, if a rather disappointed, man.

In school and undergraduate courses these objects can be brought about only by the co-operation of the teacher. He must be prepared to ' waste ' time by discussing matters other than physics with his pupils and each one of us has to decide for himself how far he can go.

But this article is concerned mainly with the corresponding problems in research. The good student will want to help in expanding the boundaries of his subject, and thus may be more than ever exposed to the danger of specialization. He may come to believe that the problem on which he is concentrating is the only problem worth while and the better he is the more fanatically he may hold this view. Conditions are, in fact, weighted considerably against his general education, since he is no longer exposed to formal courses of instruction in related subjects.

What is the answer? Of course, there is no general answer that will fit all circumstances and all students ; but one solution lies in the choice of what we may call ' borderline ' subjects in research. The purpose of this article is to show the virtues of X-ray crystallography in this light.

2. X-RAY CRYSTALLOGRAPHY

Let us first consider how the subject arose. X-rays were discovered in 1895 by Röntgen and remained a scientific puzzle for nearly twenty years ; their properties provided conflicting evidence about their nature and it seemed to be impossible to devise a crucial experiment that would provide an answer. One theory was that they were electromagnetic waves of short wavelength—so short that no diffraction grating could be made to diffract them through appreciable angles.

This problem was solved neatly and brilliantly by Laue in 1912 ; his idea provides one of the best examples in physics of the importance of a wide general knowledge combined with specialist ability. He knew of the diffraction-grating difficulty and he also knew that crystals were supposed to grow with a periodic structure. Would crystals therefore act as diffraction gratings for X-rays? Two experimental physicists—Friedrich and Knipping—tackled the problem and, after several false starts, ultimately produced a pattern of spots on a photographic plate that could be explained only by diffraction.

This experiment was one of the milestones in physics. It opened up two major fields : first, the evidence of the nature of X-rays combined with the measurement of X-ray spectral lines led to confirmation of Bohr's theory of the atom and to Siegbahn's beautiful work on the derivation of electron energy levels; secondly, the production of a new radiation, of wave-length about 10^{-8} cm, provided a new tool for the examination of matter. This subject was immediately exploited and new devices and procedures are still being produced to extend its usefulness. The subject is known as X-ray crystallography.

X-ray crystallography has been called the ' new microscopy ' and indeed the comparison is very close. Abbe in 1887 showed that the performance of the optical microscope in resolving fine detail was limited by the wave-length of light $\sim 5 \times 10^{-5}$ cm. Here was the possibility of improving its performance by a factor of 1000 !

But this improvement is not easily achieved; X-rays cannot be refracted appreciably and so we cannot make lenses and microscopes for them. In other words, we cannot produce the image of an object in X-rays as we can produce an image in light. This seems a considerable stumbling block to the use of X-rays, and much thought and ingenuity has been devoted to trying to find a way round the difficulty.

But one may well ask ' Why devote so much effort to such exotic things as crystals? '. Crystals are rather an unusual state of matter, and it might seem out of proportion to devote so much attention to them. There are two answers to this question. First, science does not ignore the unusual ; in fact, the history of science is largely based upon the exploration of the unusual ! Secondly, it is found that, on the atomic scale, crystals are much more general than one would think.

Crystals have been a subject of general interest and speculation throughout the centuries ; the crystal appears as a symbol of the strange and baffling problems that face mankind. All other natural objects are irregular ; trees and their branches form irregular patterns ; rivers flow in irregular channels ; coastlines form irregular shapes. Whenever we see straightness we suspect artificiality ; trees become telegraph poles (although I admit that they must be fairly straight to begin with !), rivers become canals, and coastlines become embankments.

But crystals grow naturally with accurately plane faces, and with accurately straight edges.

There is really only one answer to this problem. Just as a straight wall is made from bricks, so must crystals grow from the juxtaposition of identical objects—atoms, molecules, or some such units. This hypothesis must have been put forward many times, but it is usually credited to Haüy about 180 years ago ; he gave drawings (fig. 1 is an example) of how he thought that the crystalline state could arise. We now know that his ideas were essentially correct, although, of course, he could give no firm opinion about the nature of the units.

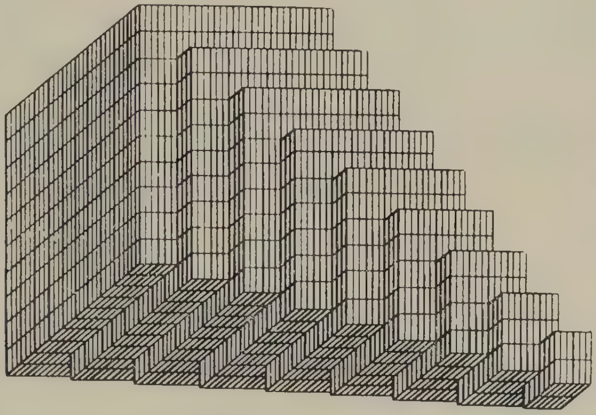

Fig. 1. One of Haüy's diagrams, showing his conception of the formation of a type of calcite. (Phillips's ' Introduction to Crystallography.' Longmans Green, 1946).

From these ideas, the shape—but not the absolute size—of the units forming a crystal could be measured, and as the study of crystals progressed even details of symmetry could be deduced. It was realized that the units of some crystals, the alums for example, must be highly symmetrical—again a very unusual manifestation of nature! The classification of crystals according to their symmetry proved to be an absorbing occupation to some scientists and they spent much of their lives growing crystals and measuring, calculating and classifying them. This is the activity now known as ' classical crystallography ', and is associated particularly with the name of Groth in Germany and Tutton in this country.

But classical crystallography is not everyone's diet ! To many it appears as a sterile subject, in which classification appears as an end in itself, unrelated to any other scientific activity.

The discovery of x-ray diffraction immediately elevated the status of the subject. It showed that nearly all solid matter is crystalline, and therefore that a study of solid matter must be preceded by a study of crystals. It enabled us to delve *within* the units that Haüy had envisaged and to show how they are composed of atoms and molecules. It enabled us to see how symmetry arose, and even to classify the symmetry of crystals in a much more detailed way than we could from measurements of external form. And the measurements of the classical crystallographers proved to be invaluable as starting places for the new researches. So we see how apparently unfruitful collection of data may prove to be of worth in an unexpected way. (I would not care to generalize from this statement ; I know too well how the collection of anything because ' it may come in useful ' can clutter up a department, and the same can happen to the workings of the mind !).

The new subject was greeted with enthusiasm. The basic ideas of Laue were soon accepted and put into usable form by W. L. Bragg. In a famous paper in 1913, he laid the foundations of the subject in a way which is a model to all research workers ; he showed how a simple idea, simply expressed, can be more useful than elaborate mathematics and extensive formulae. In 1915 W. H. & W. L. Bragg were jointly awarded the Nobel Prize for physics for their work on the structure of crystals. Several research schools soon emerged, nearly all stemming from the Braggs, and the stream of crystal structures steadily increased. Important information about increasingly complicated structures was obtained and everybody was too busy working to enquire into which branch of science his operations fell. Nor did it matter at that time ; the important task was to show that the new tool could produce results, and chemists and physicists worked hand in hand laying firm foundations for the future.

But slowly it became evident that this state of affairs could not last indefinitely. Problems arose that could not be solved by hit-and-miss methods combined with enthusiasm and perseverance. More logical methods had to be devised, and a great deal of re-thinking took place.

In fact, much of the basic theory had been worked out by W. H. Bragg in 1915, in his Bakerian lecture to the Royal Society, but it had remained dormant for nearly twenty years, since progress was so rapid without it. The complete theory would be out of place here, but a brief summary is possible. When an x-ray beam falls on a crystal, separate diffracted beams of different intensities are produced, and these can be recorded as spots on a photographic film (fig. 2). Because the crystal is three-dimensional, each beam has three integral indices, analogous to the single index representing the order of diffraction from a one-dimensional grating. Bragg's idea was that, since the crystal is three-dimensional, it can be represented by a Fourier series, also three-dimensional ; he showed that the coefficients of the Fourier series were related to the intensities of the different orders of diffraction. If the series were summed, it should give a representation of the electron density within each unit cell, and atoms would be recognized as peaks in the electron-density function. Thus it would appear that an image of the crystal should be obtainable directly from the observed diffraction data.

But now we meet a difficulty. To sum the series we need to know not only the amplitudes of the diffracted waves but also their phases. Without both these items of information the series cannot be summed. The phases, however, are not observable ; the Fourier method is therefore not of general application. It was for this reason that the method lay dormant for so long.

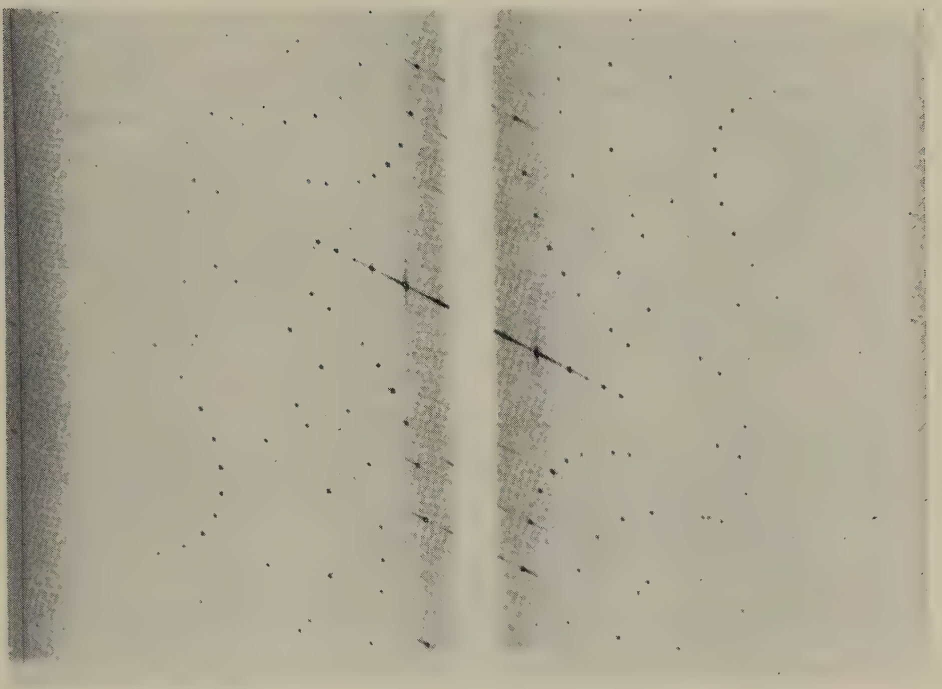

Fig. 2. X-ray photograph showing spots of different intensity. (By kind permission of Mr. W. O. Davies, Manchester.)

It was revived in answer to a definite need. The methods used in the 'heroic' period were essentially trial-and-error; one guessed how the atoms were arranged and then, by computation, found if the calculated intensities of the orders of diffraction agreed with those observed. If they did, the guess was right; if they did not, the guess was wrong and one tried again. But there is a limit to the number of parameters that one can handle in this way: each atom has three coordinates and thus ten independent atoms would introduce thirty coordinates; this is a large number to handle at once. Various aids were found—for example, the use of symmetry, and the handling of atoms in groups such as SO_4—but the problems became overwhelming. It was realized that the Fourier method—if it could be used—provided a way of handling all the parameters at once, but the basic difficulty remained.

It was W. L. Bragg in 1929 who really set the method on its feet, again not by waiting until the general problem was solved but by showing how it could be applied to special problems. He did not proceed straight to three dimensions, but showed that two-dimensional data—that is, diffracted beams with one of the three indices zero—could give useful information about the structure projected on to a plane; and he demonstrated that the summation of a two-dimensional Fourier series was not too formidable a task.

This discovery gave the subject a new lease of life. The Fourier method provided a new class of problem to which x-ray diffraction methods could be applied. Briefly, this class was characterized by the presence of an outstanding atom which gave enough information about the phases for the Fourier series to be summed, and even if the answer were not complete it was sufficiently recognizable for the necessary adjustments to be made.

In this way, Beevers and the author worked out the structure of $CuSO_4.5H_2O$ and the alums, and Robertson worked out the structure of the phthalocyanines—a beautiful piece of work in which the chemical molecule appeared (fig. 3) even though no assumptions at all had been made about its nature. A further notable step was made in 1934 by Patterson, who showed that one *could* get information without knowing the phases of the diffracted beams ; with the squares of the amplitudes as coefficients, a Fourier synthesis should give the vectors between atoms, and these should be important guides to possible structures. With these devices, the subject went racing ahead.

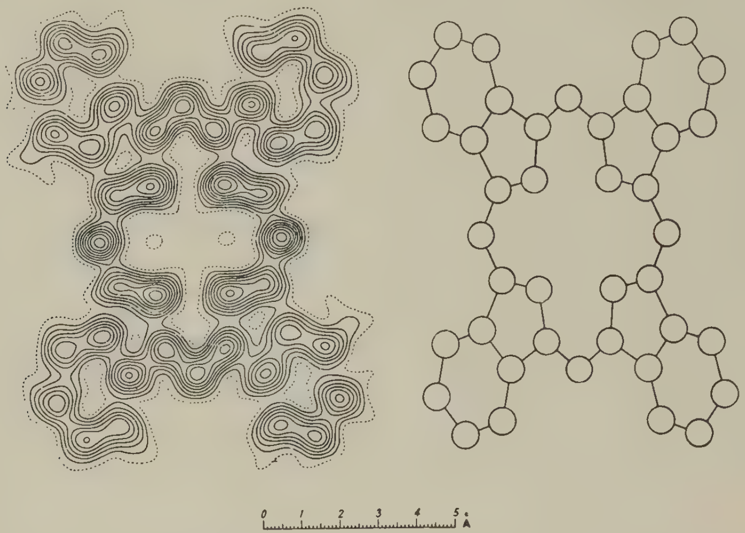

Fig. 3. The molecule phthalocyanine as computed from x-ray data. The atoms appear as peaks shown by contours of constant electron-density. A symbolic representation of the molecule is shown for comparison. (Robertson's ' Organic Crystals and Molecules '. Cornell University Press, 1953.)

At this stage, it looked as though a steady state had been reached—a triangular union involving the physical process of diffraction, the mathematical process of interpretation, and chemical interest in the results. This union, although happy, was not very satisfying to the physicists, since it was clear that the diffraction aspect was the most routine of the three ; a paper by Bernal in 1926 had solved most of the problems of assigning indices to the spots on x-ray photographs, and the really exciting work fell to the mathematician who was obtaining the results and to the chemist who was provided with information that had been thought to be unobtainable. The physicist was definitely the junior partner.

The mathematical side of the work was given a further fillip by the discovery by Harker & Kasper in 1948 that information about the phases could be obtained from the diffraction intensities themselves. Many physicists had held it to be almost axiomatic that this process was impossible ; the phase information was denied us by Nature ! However, one can seen now that this view is too simple.

Suppose that we simply guess the phases for the diffracted waves ; for many crystals they must be either 0 or π, and so the suggestion is quite reasonable. (Even so, for any ordinary number of waves the total number of combinations is very large.) The resulting Fourier synthesis is almost certain to be wrong ; it will not show clear atoms, and the electron density will, in places, be negative. Can we in some way limit the combination of phases so that we can reject in advance those that would give negative regions ? This procedure is, in fact, what the Harker-Kasper method does. Their method has been expanded and refined and now several quite complicated structures have been solved by methods that are related to it—methods which are described in severely mathematical papers, and some of which cannot be used without the aid of a digital computer.

3. THE RESURGENCE OF PHYSICS

The physicist could not, of course, be expected to be satisfied with this state of affairs, and again it was W. L. Bragg who gave a lead to the re-establishment of physical ideas. It was he who invented the term ' the new microscopy ', and he had always hankered after actually taking photographs of structures, as one does with the microscope. He had achieved this in a way in 1929 by summing a two-dimensional Fourier series photographically, projecting on to photographic paper fringes with the correct relative spacing, amplitudes, phases and orientation. This operation, however, was really experimental mathematics, not physics, and the results, although beautiful, were not very accurate.

In 1938 he had a further idea. Could not necessary fringes be produced by interference? And then the whole beauty of the idea became apparent. Young's fringes can be produced by two apertures in parallel light. One therefore sets up a simple piece of apparatus, as shown in fig. 4, and the fringes are produced by two holes in a mask placed between the lenses. For fine fringes the holes must be far apart ; the orientation of the fringes is controlled by the direction of separation of the holes ; and the strength depends upon the size of the holes. We still cannot control the positions of the fringes in the focal plane of the second lens, since each set of fringes must have a line of maximum intensity at the focus. So let us try the idea for a crystal for which the phases are all the same. We may then drill all the holes we require in the same plate (fig. 5), and the complete diffraction pattern should be a representation of the crystal structure.

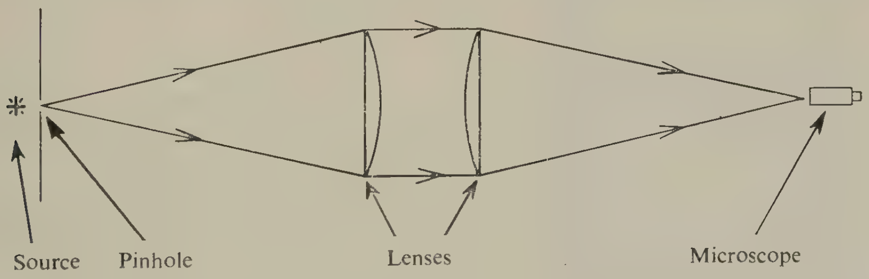

Fig. 4. Optical apparatus for producing Young's fringes. The two holes are drilled in a plate placed between the two lenses.

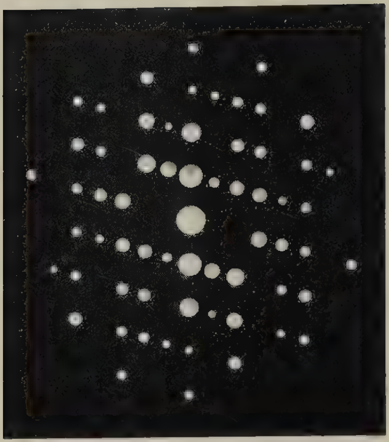

Fig. 5. Mask representing the x-ray data from diopside, $CaMg(SiO_3)_2$.

The idea was tried out for one of the projections of diopside, a mineral containing Ca and Mg atoms which, for this one projection, lie exactly over each other and so project as one atom. The result (fig. 6) showed all the atoms clearly.

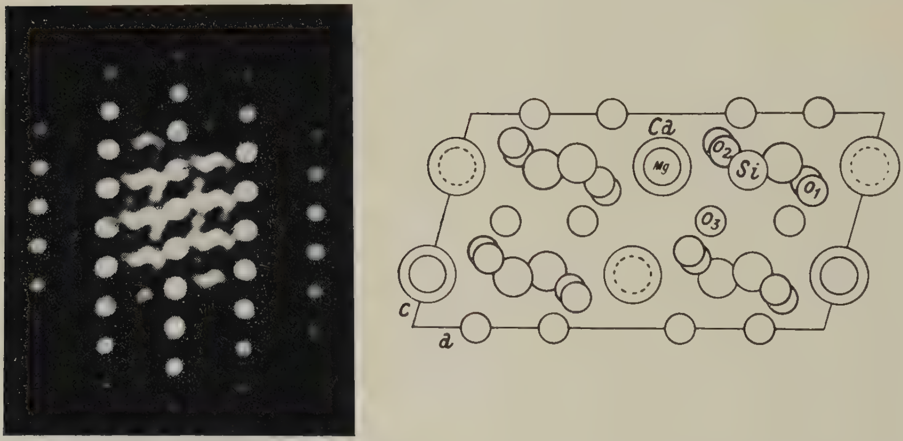

Fig. 6. Diffraction pattern from mask shown in Fig. 5. The crystal-structure projection is also shown for comparison.

But it did more than this ; it opened the way to a new understanding of the whole problem. The diffracting plate shown in fig. 5 has holes lying on a lattice ; this result was not obvious from the way in which the construction was

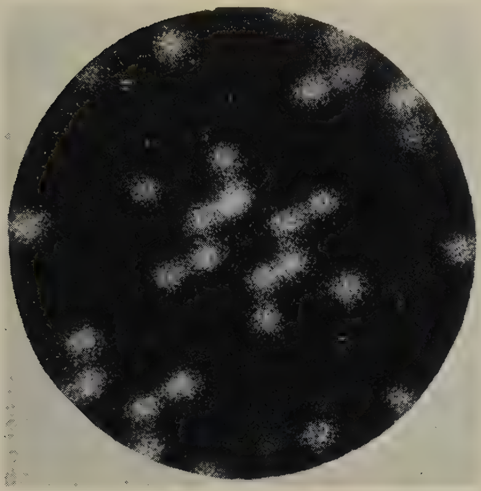

Fig. 7. Optical synthesis of molecule of hexamethylbenzene, consisting of a regular hexagon of carbon atoms with a CH_3 group at each corner. The molecule is seen slightly foreshortened.

described but it can be proved fairly simply mathematically. In fact it turned out that the lattice was what had been known as the ' reciprocal lattice ' which had been introduced by Ewald in 1921 ; it was thought then to be merely a mathematical device of no physical significance. Now it was shown that if the diffraction pattern of a crystal were represented in this way, the diffraction pattern of the pattern is the image of the crystal.

But this is not a new principle at all ; it is the foundation of the theory of the microscope laid down by Abbe in 1887. Thus the wheel had turned a full circle !

The physical meaning of Fourier synthesis became obvious. The summation of a Fourier series was simply the addition of the Abbe waves, and it had to be accomplished mathematically because it could not be carried out experimentally. In a microscope the relative phases of the waves scattered by the object are preserved ; this is the essential function of the lenses. If only we could control the phases of the waves passing through the mask in fig. 6, we could produce any image, not merely those that require all the fringes to have a maximum at the centre.

This has now been done. Taylor, Hanson and the author have, by making use of the properties of polarized light passing through mica, produced images of structures that show the separate atoms clearly. Figure 7 is one of the best examples. Despite the fact that the image is produced with light although the original scattered radiation was x-rays, the image is exactly analogous to that produced with a microscope. It is a ' new microscopy ' with a limit of resolution of 10^{-8} cm instead of 10^{-5} cm.

Unfortunately, the method still remains a scientific curiosity ; it has not yet had any serious application. The experimental work is tedious and requires considerable skill in its execution. Few people will use it when they can obtain

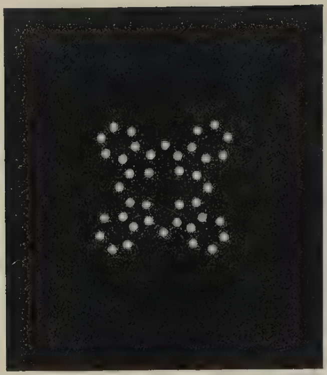

Fig. 8. Typical diffracting mask. Each hole represents an atom in the molecule of phthalocyanine.

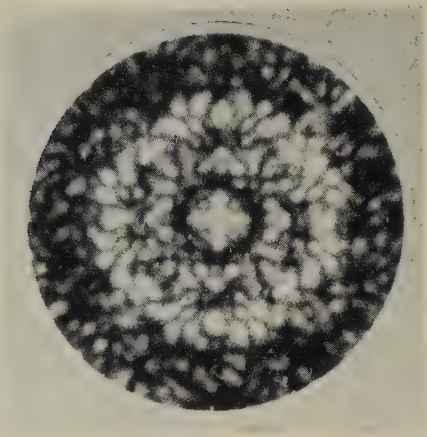

Fig. 9. Diffraction pattern from mask shown in Fig. 8.

corresponding results more quickly and easily by computation with modern computers. Nevertheless, the method is still new and can be considerably improved. When it has been found possible to simplify the procedure, considerable advantages may accrue. To be able to change phases quickly will be a considerable advantage, and one can envisage the possibility of altering the phases of the separate waves in some systematic ways until a recognizable image is produced. This is in fact directly comparable with the operation of focusing with an ordinary microscope ; one alters the relative phases of all the waves together, and when the right relationships are established the image clearly emerges.

However—as so often happens in science—the solution of one problem draws attention to more important advances in a related subject. The instrument shown in fig. 4 is essentially a large spectrometer, and with it diffraction patterns can be obtained of relatively large objects. We may therefore make a representation of a structure (fig. 8) in the form of holes in an opaque mask, and the resulting diffraction pattern (fig. 9) may be compared directly with the X-ray diffraction pattern of the crystal investigated. If the two show some correspondence (the exact details are too complicated to describe here) then the structure suggested is correct ; otherwise other masks may be punched and tried out until correspondence is obtained.

It will be seen that this process is the physical equivalent of the trial-and-error methods previously described. It may sound rather dull and uninspired, as indeed it would be if it were used solely in this way. In fact, it has made the whole of the physical basis of X-ray diffraction more clear ; it has suggested new lines of approach ; it even enables, in favourable circumstances, some details of a structure to be predicted from the X-ray diffraction pattern. It is certain that, as experience is gained, these optical-diffraction methods will play an important part in extending the range of problems that can be tackled by X-ray methods.

4. THE PRESENT STATE OF THE SUBJECT AND APPLICATIONS TO OTHER SUBJECTS

It can be seen that there is still no final answer to the problem of crystal-structure determination ; it is this fact which lends fascination to the subject. The answer may not exist, and the subject may proceed, as at present, by the accumulation of experience and the invention of new approaches. In this invention, optical diffraction is playing its part. There are, however, many who believe that mathematical methods will ultimately succeed, and thus make unnecessary any other approaches. If this success is achieved, then X-ray methods will have become a technique, which can, like the microscope itself, be used as a tool in other branches of science without full understanding of its principles. It will be interesting to see which of these predictions will ultimately prevail.

Whatever the outcome, it is certain that X-ray diffraction will continue to be used extensively in the study of the solid state. So far, its main impact has been upon chemistry. The first structure determined—that of NaCl by W. L. Bragg in 1913—showed immediately that any molecular theory of ionic crystals was untenable, in spite of the scepticism of some prominent chemists. It has had similar, if not quite so fundamental, impacts upon chemistry since then, perhaps the most recent being produced by the structures of the boron hydrides, which showed that new types of bonding must be introduced for the boron atom.

For organic chemistry the influence has not been so notable since, to a close approximation, one may say that X-ray crystallography has merely confirmed all the expectations of the chemists ; for example, the benzene ring is hexagonal and the aliphatic chain has a zig-zag formation. But even so the results are considerable. The chemist can see his molecule in much more exact detail than by any other method : he can measure the lengths of single, double and triple bonds ; he can see how the molecules can distort themselves when they are ' overcrowded ' ; he can see exactly where hydrogen bonds are—the bonds which are responsible for so many unusual properties and which are, as yet, so little understood.

And, also, the chemist now has some hope of seeing the shape of molecules that he cannot investigate by ordinary chemical methods. The extraordinary insight of Mrs. Hodgkin of Oxford enabled her and her co-workers to determine the structure of penicillin, if not before the orthodox chemists at least almost coincidentally. Her further work on the structure of vitamin B_{12} has definitely shown that, given enough effort and persistence, X-ray methods can disentangle problems that are outside the range of present-day chemical techniques.

Metallurgy follows not far behind chemistry in the extent to which it has been influenced by X-ray diffraction. In the 1930's one prominent metallurgist said that the trouble with X-ray methods was that they raised more problems than they solved ; this statement gives a vivid idea of the depth to which the methods were able to penetrate into the metallurgists' problems. The structures of metals and alloys are not usually complicated ; the more useful ones, such as iron, copper and zinc are particularly simple. But X-ray methods were able to throw a flood of light on the ways in which the atoms arranged themselves on alloying, and on their re-arrangement when they were heat treated.

For example the four supposed forms of iron—α, β, γ, and δ—were reduced to two : α and β were found to be the same, the suspected change being merely caused by the disappearance of ferromagnetism ; and δ was found to be a high-temperature reversion to α. Based upon these results, the structures of the carbon steels could be studied, and although it would be a gross exaggeration to say that they are all completely understood, at least we know the problems much more clearly.

The structure of duralumin is perhaps one of the most exciting triumphs of x-ray crystallography. Pure aluminium is too soft for ordinary use, and it was found empirically that certain additions such as copper, combined with particular heat-treatments, could increase the hardness considerably. Metallurgical evidence could not establish the cause of this effect. But x-ray examination, by Preston in this country and Guinier in France, showed that the hardness was caused by the copper atoms segregating into thin plates in the aluminium crystals ; alternated with aluminium atoms, these plates formed ‘ keys ’ which prevented slipping when a force was applied to the structure. On further annealing, the copper and aluminium atoms formed separate crystals which lost their ability to ‘ key ’ the main structure and so the hardness decreased.

While the practical results of the x-ray examination of metals have been interesting and informative, the theoretical results have been revolutionary. The picture of the metallic state is completely different from what it was thirty years ago, partly because we know so much about atomic arrangements, but more so because we can see how these arrangements affect the flow of the electrons that give the metallic state its peculiar characteristics. A process of diffraction of these electrons, akin to diffraction of x-rays, takes place within the crystal grains, and the realization of this enables considerable deductions to be made about metallic properties. The modern theory of metals is too complicated to describe here, but it forms one of the most noteworthy chapters in the application of diffraction theory.

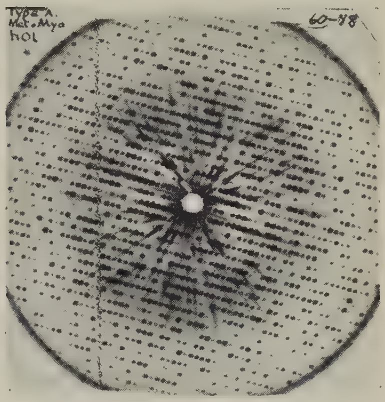

Fig. 10. X-ray photograph of protein crystal.
(By kind permission of Dr. J. C. Kendrew, *Cambridge.)*

The mineralogist has also a great deal to thank the X-ray crystallographer for. Before the advent of X-rays, the subject could not be systematized ; the silicates, which form a large part of the earth's crust, had odd compositions that did not seem to fulfil the ordinary chemical requirements of combination by simple proportions. For example, the ratio of Si to O could assume values such as 1 : 2, 3 : 7, 1 : 4, 2 : 9, and even these were not always exact.

The problem was solved by W. L. Bragg's school in Manchester ; many structures were solved, and a general pattern emerged. The structures all contained silicon atoms surrounded by approximately regular tetrahedra of oxygens, so that the ratio 1 : 4 was explained. But the tetrahedra were not necessarily isolated : they could be joined in pairs with one corner shared (Si_2O_7) or with one edge shared (Si_2O_6) ; they could be joined in larger groups, sometimes extending indefinitely through the crystal. It was found that the basis of classification was geometry, not chemistry. The SiO_4 tetrahedra could be isolated from each other ; they could link up to form chains, layers or three-dimensional networks. The properties depended very largely upon the structure ; the well-known cleavage properties of mica, for example, are caused by its possessing well-defined planes of linked SiO_4 tetrahedra which form strong layers easily separable from each other.

Other atoms of course are present, resulting in a chemically neutral material. But since some of the silicon atoms can be replaced by atoms of different valency, such as iron, with consequent changes in the numbers of other associated atoms, it will be seen what a bewildering variety of formulae can exist.

X-ray methods have also penetrated deeply into biology. Although it is true to say that the accuracy obtainable in any investigation depends upon the perfection of crystallinity—it is for this reason that the studies in chemistry have been so informative—yet even partly crystalline materials yield some information. Teeth, bone, hair and nails are all to a certain extent crystalline, and X-ray methods give some idea of the ways in which the atoms build up their structure. But the most exciting field of research at present is that concerned with the proteins—those substances that lie in the half-world between living and non-living matter, which are perfectly crystalline and yet seem to have powers of reproduction. The crystals yield a vast amount of data ; the plenitude of orders of diffraction is indicated by the large number of spots on the photograph shown in fig. 10 as compared with fig. 2. The spots contain all the information about the positions of the atoms in the crystal, but as we have already seen, there is no direct way of deriving it. Nevertheless, the methods mentioned earlier are being strenuously applied to the problem by Bragg, Perutz, Kendrew and others, and it is now definitely yielding. But the problem is vast—far greater than that of vitamin B_{12}, which is the most formidable so far successfully solved. There is a considerable gamble in this work, but if it succeeds it will be well worth the years of patient effort and persistence that have been put into it.

Examples of the incursion of X-ray crystallography with other fields, such as engineering, could be cited, but the examples already given show well enough how wide is the range of subjects to which it can be applied. Not everyone, of course, will meet all this range of subjects if he becomes an X-ray crystallographer, but it is certain that he will meet some of them, and, if he approaches the subject in the right way, he will learn a great deal of physics at the same time.

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Professor Lipson is Professor of Physics in the Faculty of Technology in the University of Manchester. He started research in 1930 in the University of Liverpool, and later joined W. L. Bragg's school of x-ray crystallography at Manchester. He accompanied Bragg to the National Physical Laboratory and then to Cambridge in 1938, and at both places continued his research on x-ray diffraction under Bragg's guidance. He left Cambridge in 1945 to go to Manchester, where he is now developing a new school of physics in addition to the well-known one in the Faculty of Science. He was elected to the Royal Society in 1957.

Magnetohydrodynamic Waves

by D. F. JEPHCOTT
A.E.R.E., Harwell

SUMMARY

The fundamental concepts of magnetohydrodynamics are presented, and Alfvén waves are discussed in terms of a simple physical picture. The applications of magnetohydrodynamic wave theories are shown to give explanations of astrophysical phenomena, and experimental evidence for the basic theory is described.

1. INTRODUCTION

Magnetohydrodynamics deals with the interaction between magnetic fields and electrically conducting fluids (e.g. liquid metals and ionized gases).

Interest in this subject arose first from astrophysical problems concerned with stellar magnetic fields, and the need to explain such phenomena as the periodic variations in the magnetic fields of certain stars, and the mechanism giving rise to sunspots. Added impetus then came from the research work directed towards the production of controlled thermonuclear reactions in the laboratory, where the whole problem virtually depends on the containment by magnetic fields of a hot, fully ionized gas.

2. MAGNETOHYDRODYNAMICS

To the physicist, the peculiar interest of magnetohydrodynamics is that it unites the two fields of hydrodynamics and electromagnetic theory. If a conducting fluid moves in a magnetic field, induced electric currents will flow in the fluid thus modifying the magnetic field, while the interaction between these currents and the field will produce mechanical forces which modify the motion. The mathematical treatment of magnetohydrodynamic problems therefore involves the equations of hydrodynamics and electromagnetic theory in a form which takes account of the interaction between the motion and the magnetic field.

It is possible to arrive at some of the conclusions of magnetohydrodynamic theory by using simple physical arguments. For example, if a conducting fluid moves relative to a magnetic field in a direction transverse to the lines of force, the fluid becomes electrically polarized. If the electric field produces currents in the fluid, they will flow in such a direction as to oppose the original motion (Lenz's Law). In the case of infinite electrical conductivity these currents would be infinitely great, hence there could be no motion of the fluid across the lines of force; the lines must therefore move with the fluid. This effect is usually described by saying that the magnetic field lines are ' trapped ' in the fluid, i.e., any fluid moving transverse to the field will carry the field lines with it. Motion of fluid along the lines of force does not affect the field.

In the practical case of a fluid with finite conductivity, relative motion between the fluid and the magnetic field can occur, so that the field lines tend to be carried with the fluid and at the same time to slip through it.

For the transport of field lines to be the dominant effect the following condition must be satisfied :

$$4\pi\mu\sigma l v \gg 1$$

where μ is the permeability of the fluid, σ is the conductivity of the fluid, l is a typical dimension of the field, and v is the velocity of the fluid.

This condition is satisfied in stars which have material of high electrical conductivity, magnetic fields, and enormous dimensions. Hence in the interior of stars the magnetic fields can be almost perfectly trapped in the material. In the laboratory, on the other hand, conductivities are lower and the dimensions are limited by practical considerations, so that it is difficult to achieve a high degree of trapping of a magnetic field in a conducting fluid.

2.1. *Alfvén Waves*

An important development in the study of magnetohydrodynamics occurred in 1942 when Alfvén predicted the existence of a type of wave motion produced by the coupling between electric and mechanical forces described above. He showed theoretically that local motion of a conducting fluid in a magnetic field can be transferred to the surrounding fluid, and that the state of motion is transmitted as a transverse wave along the direction of the magnetic field. These waves are known as magnetohydrodynamic waves, or Alfvén waves. Alfvén gave the following simple physical picture of magnetohydrodynamic waves:

A magnetic field behaves mechanically as if the lines of force were subject to a tension of $\frac{\mu H^2}{4\pi}$. Also, if the field is trapped within a conducting fluid, then the lines of force have associated with them the density of the fluid. The lines may therefore be considered as material strings having both a tension and a density, and like ordinary strings they may oscillate. Simple dynamics shows that the velocity of propagation of transverse waves along a stretched string is given by

$$V^2 = \frac{\text{Tension}}{\text{Density}}.$$

Hence, using this analogy, the Alfvén wave velocity should be given by

$$V_A{}^2 = \frac{\mu H^2}{4\pi\rho}$$

where V_A is the Alfvén speed, H is the magnetic field strength, and ρ is the density of the fluid.

This is in fact the same as the expression arrived at by Alfvén using a theoretical analysis based on the equations of magnetohydrodynamics. The velocity quoted above strictly applies only to the case of wave propagation in an incompressible fluid with infinite conductivity, zero viscosity, and no boundaries, but the theory has been extended to allow for the effects of electrical resistance, viscosity, and other physical parameters which are important in practical cases.

3. THEORY OF SUNSPOTS

Long before the existence of magnetohydrodynamic waves was demonstrated in the laboratory, Alfvén and other workers had applied the new concept in attempts to explain certain astrophysical phenomena.

Perhaps the most interesting of these theories are those advanced by Alfvén, and later by Walén, to explain the mechanism by which sunspots are produced. Measurements of the Zeeman effect in Fraunhofer lines indicate that the sun has a magnetic field (of a few gauss) and, since the solar interior consists of highly ionized matter, the sun can be considered as a good electrical conductor out as far as the photosphere. Conditions are therefore ideal for the existence of magnetohydrodynamic waves.

Alfvén's theory assumes periodic bursts of convection in the sun's core, causing a hydrodynamic vorticity which initiates magnetohydrodynamic waves. The waves travel in the form of closed rings along the direction of the sun's magnetic field and eventually reach the photosphere after about 40 years.

At the photosphere the rings are reflected, and the two areas of intersection of a ring with the surface of the photosphere are seen as sunspots. Alfvén thus explains the high magnetic fields associated with sunspots and accounts for the fact that they occur in pairs with opposite magnetic polarities. The theory is also successful in explaining such phenomena as the relative magnetic polarities of sunspots in opposite hemispheres, and the motion of sunspot zones towards the solar equator.

4. Further Applications in Astrophysics

A magnetohydrodynamic wave may be damped by energy losses due to the resistance and viscosity of the surrounding medium or to the collisions of ions with neutral gas atoms, the energy lost by the wave appearing as heat in the surrounding medium. It has therefore been suggested by several authors that the outer layers of the sun may be heated by the damping of magnetohydrodynamic waves which originate in the turbulent regions of the photosphere. Alfvén suggested that the heating of the corona might be due to resistive damping, but this mechanism alone is probably insufficient to explain the high temperature (about 10^6 °K) believed to exist in the corona. Piddington has investigated theoretically the effect of damping by ion-neutral collisions, and shows that heating of the chromosphere could be explained in terms of this mechanism.

Piddington has also suggested that the irregular wisps of light observed in the Crab Nebula are caused by magnetohydrodynamic waves compressing the ionized gas through which they pass, the compression being sufficient to produce visible radiation from regions of higher density.

It is also possible that the explanation of the enormous energies acquired by cosmic ray particles may be found in Fermi's idea that the particles are accelerated in interstellar space by their interaction with the variable magnetic fields of magnetohydrodynamic waves.

5. Experimental Work

Because of the importance of the application of Alfvén's theory in astrophysical problems, interest arose in laboratory experiments designed to demonstrate the existence of Alfvén waves and to investigate the conditions under which they propagate.

The first successful work was reported by Lundquist (1949), who demonstrated the existence of magnetohydrodynamic waves in mercury. His apparatus (fig. 1) consisted of a vertical stainless steel cylinder containing mercury, with a magnetic field of up to 13 000 gauss parallel to the axis. This field strength corresponds to wave velocities of the order of 1000 cm/sec in mercury. The base of the cylinder contained a horizontal disc, fitted with radial strips, which was made to perform small oscillations about the axis. Torsional magnetohydrodynamic waves were thus set up which travelled towards the free surface of the mercury, where they were reflected. The oscillations were detected by means of an optical system with a mirror floating on the mercury surface. Lundquist measured the wave velocity in terms of the difference in phase between oscillations at the base and at the surface, and measured the attenuation from the ratio of the amplitudes. His numerical results were sufficient to show that the theory was at least qualitatively correct.

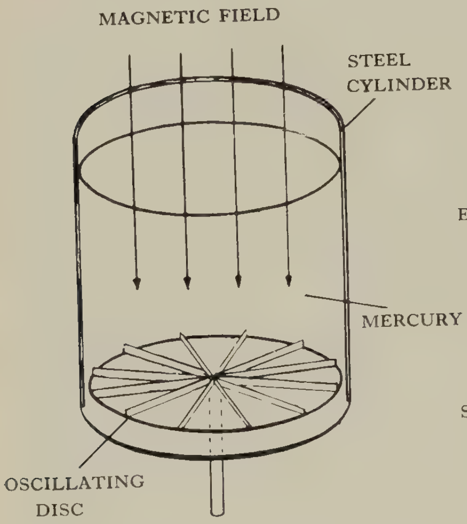

Fig. 1. Lundquist's apparatus for demonstrating magnetohydrodynamic waves in mercury.

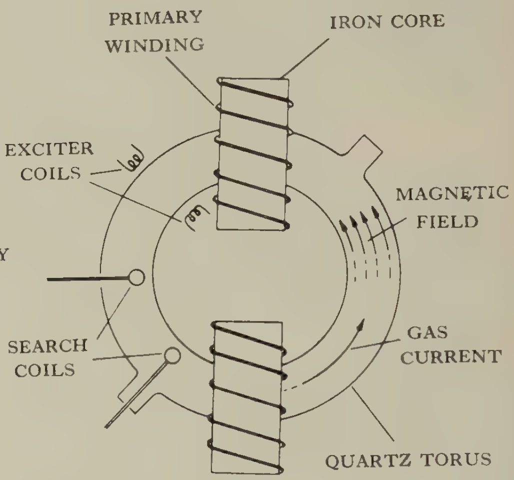

Fig. 2. Scheme of apparatus for investigating magnetohydrodynamic waves in a toroidal gas discharge tube.

A similar experiment was carried out by Lehnert (1954), using liquid sodium as the conducting fluid. This has a lower density and higher conductivity than mercury, resulting in a higher wave velocity and lower attenuation. By refinements in theory and experimental technique, Lehnert was able to obtain better quantitative agreement with theory.

Meanwhile, in the field of research directed towards the attainment of controlled thermonuclear reactions in the laboratory, physicists were investigating the stabilizing effect of magnetic fields on electrical gas discharges, and several workers reported the observation of oscillations in magnetic fields which might be evidence of Alfvén waves propagating in the ionized gas of the discharge.

Experiments were therefore carried out (Hardcastle and Jephcott 1959, Wilcox 1959) to excite Alfvén waves in a gas discharge, and to investigate the physical properties of the waves in these conditions.

At A.E.R.E. Harwell we have been working with a toroidal gas discharge tube (i.e., a tube shaped like the inner tube of a car tyre) which acts as a single secondary turn of a pulse transformer (fig. 2). The tube is filled with a monatomic gas at 0·1 mm of mercury pressure, and when a condenser bank is discharged through the primary winding the gas is ionized and passes a current of 10 000 amp. A magnetic field of up to 14 000 gauss is applied in the torus parallel to its circular axis. When the gas current reaches its maximum value, an oscillatory electric current is passed through two small coils close to the torus wall. This induces in the ionized gas small local oscillations which, because of the presence of the magnetic field, propagate as Alfvén waves round the discharge tube, the velocity of propagation in argon being about $2 \cdot 10^7$ cm/sec at 14 000 gauss. The waves are detected by means of magnetic search coils spaced along the tube, the velocity being given by the time delay between signals from successive coils and the attenuation by the relative amplitudes of the signals.

Experimental measurements of the wave velocity and attenuation over the available range of magnetic field strengths show close agreement with theory, though some of the detailed phenomena observed are not yet clearly understood.

Experiments such as those described above do not only demonstrate the existence of Alfvén waves, but also confirm the validity of the fundamental magnetohydrodynamic equations. This is important because these equations are used extensively in treating other problems.

6. Conclusion

The theory of magnetohydrodynamic wave propagation has produced new and interesting explanations for some of the complex phenomena observed by astrophysicists. Although only poor approximations to the physical conditions existing in the stars can be set up in the laboratory, experiments have indicated the essential correctness of the theory. It follows from this that the experimental study of Alfvén waves can provide a valuable means of obtaining information about the fundamental physical processes occurring in ionized gases.

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Atoms and Molecules

A FRIDAY EVENING DISCOURSE DELIVERED ON NOVEMBER 20 1959

by SIR LAWRENCE BRAGG
Director of the Royal Institution

During the last six years a new scheme has been initiated in the Royal Institution which we term the ' Schools Lectures '. These lectures are given twice weekly from October to June, and are attended by boys and girls from about 500 grammar schools and other schools in which science is taught to an advanced level. Most of the lectures are for young people from the top science forms, but each year we have one series for younger boys and girls. It is hoped in this way to arouse an interest in science at a stage of their studies when they have not yet necessarily begun to specialize. The discourse this evening is one of these latter talks. I shall therefore ask the members of my audience to consider themselves as being 14-plus, and give this talk as it would be delivered to the young people, in the hope that it will be interesting as a sample of what we are trying to do.

The main idea behind this particular talk is to make the boys and girls more familiar with the concepts of atoms and molecules. It is hardly possible to avoid using these concepts in any scientific talk, however popular. They do, however, present difficulties to the average listener who has no expert scientific knowledge. This is partly because atoms and molecules are so much smaller than anything which can be seen, and partly because the structure of an atom or molecule is so unlike that of any little piece of mechanism we can handle or visualize. The demonstrations were planned, therefore, to lessen the strangeness of this tiny world, by demonstrating large-scale effects which could be linked directly to the ultimate building particles of matter. The concepts are of basic importance.

An interesting thing one can tell the young people at the start is that our knowledge of the nature of atoms and the forces which bind them together into molecules is so recent. The atomic theory was formulated by John Dalton a century and a half ago. He pictured the elements as being composed of identical small particles, the atoms. In compounds, the ultimate particles are in identical small groups of atoms called molecules, bound by forces of some kind into characteristic family parties. The molecule of water is always one atom of oxygen combined with two atoms of hydrogen, that of carbon dioxide two atoms of oxygen combined with one of carbon, and so forth. This postulate was based on the discovery that the relative proportions of elements by weight in a compound could be simply explained, by assigning relative weights to the atoms in it. Dalton's theory required no assumptions about the actual size or mass or structure of the atoms ; it merely postulated their existence and their relative masses. When I was a young student, little more was known ; atoms and molecules were still quite vague abstractions. In fact some well-known scientists still did not believe that their existence had been proved. All our intimate knowledge of them has been gained in the last fifty years.

The first step is to realize that their minuteness is not quite so terrifying as it might at first sight appear. The point is made with the help of a scale of sizes on a large diagram, each step in the scale being associated with objects ten times smaller in dimension than at the step above. The diagram is on a sheet of steel, and illustrations of objects on the steps can be clamped on by magnets as the argument proceeds. Experience has shown that this gives a more vivid impression than a ready-made diagram. The top step of the scale is associated with objects about an inch across, represented by large crystals of sugar which they can all see. Coffee-sugar crystals represent going down one step, they are about ten times as small ; then comes castor sugar, then icing sugar, so we have gone down three steps of the scale in a painless way with quite familiar objects. The point to be made is that we only have to go down five more steps, and we shall have arrived at the molecules of which the sugar crystals are composed.

To see the form of the icing sugar grains, one must use a lens, and anything smaller requires a microscope. A microscope image of a flea represents an object seen in considerable detail, and one of a bacterium something which is already rather diffuse because the microscope is nearly at its limit of resolution. Up to this point one is still in the range when ‘ seeing is believing ’, but beyond it the difficult point is reached where things can no longer be seen with any optical aid. Starting with 1 in. one has gone down five steps to objects 100000 times as small. It seems worth while explaining that light fails to be of any further help, because the details of the object are closer together than the wave length of light. The object itself is not being magnified by the microscope, one is only magnifying a storm of light waves created when light falls on the object, and the details of this storm cannot be finer than the length of the waves which create it. A demonstration which seems to drive this point home is to sign one’s name over about 2 ft, first with a crayon and then with a distemper brush. It is often hard to predict what success a demonstration will have ; this particular one seems to tickle the fancies of the young people, it goes down surprisingly well!

The pictures obtained with the electron-microscope serve to carry on the story. The very detailed picture of a bacterium, the bacteriophage, the virus are good examples. A virus particle can be claimed to be an extremely large molecule. An electron micrograph of a protein crystal shows molecules which are still very large, but now only about ten times as great each way as the sugar molecule. The electron-microscope pictures are so closely analogous to those obtained with an optical microscope that it is justifiable to claim this wonderful new instrument as one which enables us to ‘ see ’ large molecules.

The final stage has to be touched on very lightly. So far it is only possible to deduce the arrangement of the atoms in the molecule by x-ray diffraction. One can show an x-ray diffraction photograph, preferably one with four-fold symmetry, and then ask the young people to look at a bright point source of light on the lecture bench through their handkerchiefs. The diffraction pattern produced by the handkerchief looks very like the x-ray pattern. It can be explained that they are produced in the same way, though the lattice pattern is due to waves 10 000 times shorter, and that the nature of the pattern can be deduced from the nature of the diffraction. The model of a sugar molecule which they are shown completes the story.

There are certain demonstrations of large-scale effects which, to my mind, form a bridge between everyday experience and the minute world of atoms and molecules, and which show convincingly that matter is composed of discrete particles. The first of these is Lord Rayleigh's famous experiment which gave an approximate idea of the absolute size of a molecule. The young people often know something about surface tension, and one can do a few simple experiments to remind them that a liquid behaves as if it had an elastic skin, explaining that this effect is due to the pull exerted on the surface molecules towards the interior of the liquid. It is then pointed out that since the tension is a surface effect, its strength depends on the nature of the surface. If a drop of oil is placed on a water surface, it rapidly spreads as a thin film over the surface and the tension is reduced. Three sticks of wood can be laid on a clean water surface in a triangle, their ends adhering by capillary attraction. A drop of detergent, which spreads in the same way as oil, is allowed to fall inside the triangle, when the sticks fly apart with a considerable velocity. This is a good example of the strength of the tension. Rayleigh used the camphor movements to indicate the area over which the drop spread. Particles of camphor scraped by a knife on to a clean water surface dash about in all directions, because of the difference in tension of the camphor-contaminated water in their wakes behind them and the strong clean water surface in front to which they are drawn. An oil film kills the movement. Rayleigh guessed that the oil forms a monomolecular layer on the water, and by measuring the area over which a known amount of oil spread, he was able to establish that this layer is about 50×10^{-8} cm thick.

Another piece of evidence is provided by the Brownian movements. Minute particles like those of smoke in air, or the fat droplets when a small amount of milk is added to water, are seen under a high-power microscope to be in constant agitation. Aquadag, which is a suspension of colloidal graphite, shows the effect well if a small quantity is added to water. The agitation is due to the bombardment of the particles by the molecules randomly striking them ; they are sharing in the general kinetic energy of the molecules and if they are sufficiently small, the velocities are appreciable. One can illustrate the nature of the effect by making an artificial gas of a number of small ball-bearings about one millimetre in diameter. These are placed in a watch glass and magnetized by holding a strong permanent magnet beneath them. The glass is then placed within a Helmholtz coil, fed with 50-cycle current through a variac. As the current is increased, the bearings break away and roll about in all directions, because they each make a complete revolution in 1/50 sec. A small wad of paper placed in their midst is knocked about in a way which simulates precisely the Brownian movement. This is a convincing way of showing that a gas cannot be a continuum, but must be composed of a number of particles flying about in all directions.

Finally, the crystalline state provides evidence that matter must be composed of particles. A pretty experiment devized by Professor Bernal explains the nature of a crystal. A number of marbles of uniform size, poured into a container of no special shape, make an irregular pile. If, however, one makes a container of four equilateral triangles, so that it has the shape of the lower half of an octahedron, and pours the marbles into it they arrange themselves regularly and pile up so as to complete a perfect octahedron, which can be compared with one of the octahedra so readily grown with alum. A crystal is a regular pattern of identical units, the molecules. If a piece of muscovite mica is cleaved, and a drop of ammonium iodide solution is allowed to dry out on the fresh surface, one gets tetrahedral crystals all pointing the same way, because the ammonium iodide fits itself to the underlying pattern of the mica surface just as the marbles crystallize in the regularly shaped container.

Finally, an attempt can be made to give the young people some idea of the nature of the forces which bind the atoms together into molecules. It can, of course, only be a very preliminary and general idea, which will become more precise as they pursue their scientific studies. The periodic table gives the clue. An atom cannot be compared to a tiny version of mechanical objects with which we are familiar, it is not a little solid joined to other atoms by hooks or bolts. It is like a small planetary system, with electrons whirling around a central nucleus, each atom in the ascending series of the periodic table having one more electron than its predecessor. The extreme change in chemical properties from one atom to the next, and the reappearance of similar properties in a periodic way, can only mean that the dance of the electrons around the nucleus is a formal measure governed by precise rules of symmetry, and not an irregular rout. One can compare the union of sodium with chlorine to a joining forces of two eightsome reel parties, one of which is short of a partner for the complete dance figure, and one of which has an extra wallflower who cannot take part in the dance. By sending a partner from one to the other, both parties achieve a more symmetrical state, which as in a crystal is the ideal form for the lowest energy state. This is, of course, only a pretaste of the more complete explanation they will get later, but the young people do seem to appreciate the conception, already illustrated by the crystal, that matter tends to take up its most symmetrical form and that atoms unite into molecules because, by exchanging or sharing electrons, they can build a neater structure of higher symmetry.

The purpose of these demonstration lectures is not to give the boys and girls information, but to give them concepts in terms of which they can think. They seem to remember so much better what they see than what they hear or read, hence all the emphasis is on experiments, working models, or attractive diagrams. We are indebted for help in planning the lectures to the science teachers, who advise us as to how we can best supplement what the young people learn at school.

The Physical Society Conference on Nuclear Physics at Liverpool, March 1960

by L. L. GREEN and H. MUIRHEAD
University of Liverpool

The Physical Society sponsors two conferences each year on nuclear physics. The conferences are organized jointly by the Physical Society and the establishment acting as host—usually a University Physics Department. The main objectives of these conferences are to promote regular meetings between the nuclear physicists of this country, to enable them to hear about and discuss the latest advances in their subject, and to offer young research workers the opportunity of making the first presentation of their work in public. The second objective is attempted by inviting certain senior physicists to give papers of a review nature; these normally last about 30 to 45 minutes. The invited papers are then followed by short contributions of 10 to 15 minutes duration—these are mainly given by research students or junior fellows. The first of the conferences for 1960 took place in Liverpool on 30 and 31 March.

The opening paper was given by Dr. A. W. Merrison (CERN). Dr. Merrison described the anxious and exhilarating period when the CERN proton-synchrotron was being brought into operation. The machine shows every promise of being highly successful, exceeding the expectations of its designers. At present protons are being accelerated to an energy of 25 GeV ($1\ \text{GeV} = 10^9$ electron volts of energy). The acceleration process yields groups or pulses of protons, and the designers had hoped to obtain 5×10^9 protons per pulse, in fact it now yields 5×10^{10} protons per pulse.

At present much of the work on the machine is devoted to engineering development so that it will run regularly and with the minimum of trouble. Some nuclear physics studies have been started, however, since a knowledge of the numbers and types of elementary particles emerging from a target when it is struck by a beam of 25 GeV protons is essential for the programme of beam engineering. Beam engineering involves the production and transportation of beams of particles with the maximum attainable purity and intensity to convenient points at which the nuclear physicist may perform his experiments.

At present, therefore, the nuclear physicists are studying problems such as what fraction of the particles of negative charge emerging at an angle from the target, say x degrees, with respect to the proton beam, and with a momentum of y GeV/c, consists of pions, K-mesons and anti-protons.

The preliminary results show that most of the particles leaving the targets are pions (particles of mass 273 M_e) but that a significant fraction of the beams consists of kaons (particles of 966 M_e) and anti-protons (protons of negative charge). The kaons constitute about 30 per cent of the particles leaving the targets, and the anti-protons a few per cent. The relative abundance of these particles is of extreme importance for later experimental work, since the problem of producing pure beams of particles of one type with a specific energy becomes progressively easier as the fraction of particles of that type increases in any beam.

The CERN synchrotron will have great advantages over the 6 GeV synchrotron at Berkeley, for example, in the performance of experiments on the properties of the anti-protons. The anti-proton beam of the latter machine is about 10000 times weaker than the pion beam.

Dr. Merrison's contribution was followed by one from Dr. P. T. Matthews (Imperial College of Science and Technology), who described some of the topics of current interest to theoretical physicists working on the theory of elementary particles.

He first discussed the possibility of detecting the nuclear reactions induced by neutrinos of high energies. The neutrino (a particle of zero mass and charge, but spin of $\frac{1}{2}$) has a notoriously low probability for interacting with nuclei. Cowan and Reines detected the following process, which is the inverse of β-decay

$$\bar{\nu}+\mathrm{p}\rightarrow\mathrm{n}+\mathrm{e}^{+}$$

using the intense anti-neutrino beam from a reactor. The neutrinos have energies of the order of an MeV. The measured cross-section for this process is about 10^{-44} cm^2; in order to appreciate the smallness of this figure (which is a measure of probability of detection of a nuclear process) it should be borne in mind that typical cross-sections for the nuclear interaction of two nucleons are about 10^{-26} cm^2.

Various theoretical physicists have recently pointed out, however, that if neutrinos of high energy can be produced, then the cross-sections induced by them would be much larger than those of neutrinos from a pile. At energies of a few GeV, for example, the cross-sections would be about 10^{-35}cm^2.

The large accelerators of the type being brought into operation at CERN produce intense beams of pions which decay to yield neutrinos, for example:

$$\pi^{+}\rightarrow\mu^{+}+\nu\ .$$

The possibility of producing neutrino interactions at high energies is therefore being seriously examined.

Another topic dealt with by Dr. Matthews is really in the sphere of atomic physics, but nevertheless is of great interest to the physicist studying elementary particles.

The basic physical arguments used in establishing the properties of the particles are normally very simple. For example, the conservation of energy and momentum may be used to determine the mass of particles, and the conservation of angular momentum and parity to determine their spin and parity. (The latter concept is unique to quantum mechanics, basically it is concerned with whether the wave function describing a physical system changes sign or not when the co-ordinates of the system are reflected.)

As an example of the determination of parity we may consider the identification of the following process when mesons are brought to rest in deuterium.

$$\pi^{-}+\mathrm{d}\rightarrow\mathrm{n}+\mathrm{n}\ .$$

In this reaction angular momentum and parity must be conserved. By making a justifiable assumption that the capture of the π^{-} meson takes place when it is an S-state about the deuteron, it may be shown that the pion possesses odd parity relative to the two nucleons.

Ever since beams of K^- particles have been available from machines, physicists have done experiments of the type in which K^- mesons stop in hydrogen or deuterium and the reaction products have been examined. Unfortunately, interpretation of the results is more ambiguous than for π^- mesons. This is because of uncertainties concerning the angular momentum of the K^- meson relative to the nucleus at the time of capture.

A year ago Day, Snow and Sucher advanced an argument which suggested that capture of kaons could be predominantly from the S-state. Briefly they pointed out the tightly bound kaon-proton system looked roughly like a neutron as far as electrical charge is concerned. This system can penetrate the ordinary hydrogen atom, and reach the intense electrical fields close to the proton. In this region Stark mixing can take place, and this process favours transitions to the lowest L values in the state.

This conclusion has been queried by Adair, however, who claims that Day, Snow and Sucher have not taken the conservation of angular momentum into account in a sufficiently rigorous manner. Adair suggests that about 20 per cent of the kaons undergo capture from the P-state.

Dr. Matthews urged his fellow theoretical physicists to clear up this important problem as quickly as possible.

Many important topics were presented in the short contributions, but space does not allow us to give an adequate review of all of them. Two topics were especially worthy of note. Firstly, now that pion beams of high energy are available the possibility of examining the pion-pion interaction is now feasible in principle. Work, both experimental and theoretical, on this topic was reported. The problem is not an easy one, however, since nucleons are normally present in reactions which are at present feasible, for example

$$\pi^+ + p \rightarrow \pi^+ + \pi^+ + n\,.$$

Needless to say, the strong pion-nucleus interaction complicates the problem.

The second topic concerns an experiment on the *g*-factor for the muon in progress at CERN. The accurate measurement of *g* constitutes an important test for quantum electrodynamics. This was reported by Farley, briefly muons are captured into a circular orbit in a magnetic field. The spin of the muon precesses with a frequency

$$\omega_s = \frac{g}{2}\,\frac{eH}{mc}$$

whilst the particle executes a circular orbit of frequency

$$\omega_c = \frac{eH}{mc}\,.$$

Thus if *g* is not equal to two, the spin of the muon will precess about its direction of motion. The detection of such a precession is feasible by virtue of the failure of parity in the muon decay.

Farley reported that work on this problem is proceeding satisfactorily, and that the same apparatus may be used to look for the possible existence of an electron dipole moment for the muon (if the muon possesses a dipole moment the magnetic field would cause the spin to move out of the plane of rotation). No evidence for the existence of a dipole moment was found.

The main talks on nuclear structure were given by Dr. A. E. Litherland of Atomic Energy of Canada, who is spending a year at Oxford, and by Professor B. H. Flowers of Manchester University.

Atomic Energy of Canada at Chalk River has had for the last year one of the first tandem Van de Graaff generators, and Dr. Litherland discussed the work which has been carried out with this machine. The tandem accelerator consists of two electrostatic accelerators in series. There is one central electrode at a potential of 5–6 million volts. The ion source produces negative ions which are accelerated to the central high voltage terminal, where the electrons are stripped off the ion, which is then positively charged, and accelerated back to earth. The resulting protons consequently have fallen through twice the voltage of the terminal and a terminal voltage of 5 MeV thus produces 10 MeV protons. The beam produced by these accelerators is continuous, well collimated and well defined in energy. The energy is easily varied from 2 to 10 MeV. A further important advantage is that the ion source is outside the pressure vessel. Besides accelerating protons and deuterons to 10 MeV the machine can be used for accelerating other ions. For instance 35 MeV 0^{16}_{6+} ions have been obtained both at Chalk River and Aldermaston and in principle many other heavy ions may be accelerated. This machine is an attractive and very useful instrument for studying nuclear reactions and nuclear structure.

The easy energy variability and good energy resolution of this machine has been used at Chalk River to study the mechanism of d,p reactions. A study of the Si^{28} (d,p γ) Si^{39} reaction by E. Almquist, D. A. Bromley and J. A. Kuehner was described by Dr. Litherland.

It has been known for some time that reactions induced by deuterons often proceed by only the proton or neutron of the deuteron interacting with the nucleus, the other particles being virtually unperturbed. The main features of the angular distribution of the reaction product can be explained on this picture and they are often used for determining spins and parities of nuclear states. However, if one is interested in more detailed features, such as the polarization of the outcoming particle and subsequent gamma rays from the excited nucleus, then it is necessary to take into account the interaction of the unperturbed particle with the nucleus. To study these effects Dr. Litherland described how the Chalk River groups have carried out systematic studies of the p,γ correlation, and the angular correlations show that the simple theory with plane waves for the ingoing and outgoing particles is inadequate to explain the observed correlations although the deviations are not large. It was possible to describe the results using the distorted wave theory of Huby, Refai and Satchler. The agreement with the plane wave theory satisfactorily improves with increasing energy.

One important use of the heavy ions produced in the tandem accelerators is the use of these for Coulomb excitation. In this process the nucleus is excited by the Coulomb field, which one knows about, rather than through any nuclear interaction. Heavy ions have an important advantage that much higher incident energies can be used before the bombarding particle penetrates close enough to form a compound nucleus. In consequence states of rather higher energy can be excited by heavy ions without the complications of nuclear interaction than is possible with protons or α-particles. Dr. Litherland discussed the results on the excitation of the 1·78 MeV first excited state of Si^{28}. These studies give the electric quadrupole transition probability for this state which is an important quantity for comparison with theoretical models.

Coulomb excitation studies with heavy ions have also been carried out at Harwell and Aldermaston and Dr. Eccleshall, in a short paper, described the work at A.W.R.E. on the double excitation 4 + levels in Sm^{152}, Si^{154} and W^{186}.

The use of heavy ions for reaction studies permits some interesting lifetime studies. If an excited state is formed in a reaction such as Be^9 (0^{16}, α n) Ne^{20}, the Ne^{20} will have a high recoil velocity and γ-rays from it will show larger Doppler shifts. This effect may be useful for studying lifetimes of some nuclear states.

In a short paper before Dr. Litherland's talk Mr. R. Batchelor, of A.W.R.E., who has recently spent a year at Chalk River, described the work there and the inverse giant resonance in photodisintegration. The cross-section for photodisintegration of a nucleus shows a very large resonance peak whose energy varies throughout the periodic table. There are two current theories for this dipole resonance in nuclei, the independent particle model and the collective model in which the nucleus as a whole is polarized. γ,p studies are experimentally difficult to carry out because there are few monoenergetic γ-ray sources and nearly all studies use a continuous bremsstrahlung spectrum. The cross-section for the γ,p reaction is related to that for the inverse p,γ reaction, which should also show the resonance at the appropriate proton energy. The inverse reaction should be capable of yielding much more precise information. A tandem machine with easily variable proton energy is useful for studying some of these inverse reactions, and at Chalk River the inverse giant resonance in B and Al has been observed. In the base of boron the ground state γ-ray has an angular distribution which is consistent with the independent particle picture although this is reported not to be the case for the γ-rays from Si^{28} following proton capture in Al^{27}.

Dr. A. A. Jaffe, of Manchester University, presented some results on H^3,p reactions. The theory of these reactions, similar to simple deuteron stripping, has recently been worked out by Dr. H. Newns of Liverpool. Dr. Jaffe showed results for a number of light elements using 5 MeV tritons. Good agreement between the experimental angular distributions was obtained for a number of levels, and the distributions can be used to give the parity of nuclear states. These reactions may prove as useful in nuclear spectroscopy as the deuteron stripping reactions have. Similar agreement with this simple theory is reported for He^3,p reactions from Aldermaston.

An important technical development in nuclear physics recently has been the use of solid state devices for particle counting. Dr. Dearnaley, of A.E.R.E., described recent results they have obtained using germanium and silicon to count alpha particles and protons. At the moment there is a limitation in the use of these counters for total energy measurements in that the depletion layers used for counting are quite thin. Protons of energy up to 6 MeV may be counted with germanium and up to 2 MeV with silicon. The counters have high resolution and small physical size and a pulse rise time of 10^{-9} seconds. These features make them attractive to nuclear physicists and a great deal of work is being carried out in many laboratories on them. They have been used extensively for detecting heavy ions at Chalk River.

The main talk of the conference on the theory of nuclear structure was given by Professor Flowers of Manchester University. He singled out several topics for discussion. One of these was the work of Soper on nuclear forces and nuclear models. Usually in this work a plausible form of nuclear force is assumed and an attempt is made to calculate the nuclear energy levels. Soper took the opposite line and tried to see what one could find out about the nuclear force given the energy levels. The outcome of this work is that the type of nuclear force one should use in independent particle model calculations is fairly well established although it is not clear of course how this force relates to the two body nuclear interaction. Another topic discussed at some length by Professor Flowers was the recent work of Brown and co-workers at the giant resonance of photodisintegration. One of the difficulties of Wilkinson's independent particle theory of the giant dipole resonance is that it predicts too low an energy for the resonance. Brown has shown how the effect of the correlation between the particle and the hole in the closed shell pushes the dipole state to a much higher energy.

A session of the conference was devoted to work in this country on recoilless absorption of nuclear γ-rays. Unti lrecently the resonant absorption of γ-rays by nucleus has been quite difficult to observe because the recoil Doppler shift is normally greater than the width of the resonance. Mössbauer a year or two ago showed that, under appropriate conditions, a nucleus may emit a γ-ray from a position in a crystal lattice and the recoil, taken up by the whole crystal lattice, thus producing recoil-less emission with virtually no Doppler shift. As Dr. Schiffer pointed out, this may be a new phenomenon to nuclear physicists but it is well known to X-ray crystallographers, and is described in standard textbooks on that subject. The line is very narrow and comparatively simple experiments can be carried out in which γ-ray energy change of 1 part in 10^{12} can be detected. Professor Moon reviewed this subject and introduced some of the work carried out at Birmingham.

Dr. Schiffer described the Harwell work on the gratitational red shift using this effect. Unfortunately the result of this experiment turned out to be uncertain because of an effect found at Harvard and pointed out independently by a Trinity undergraduate, Mr. Josephson. The precise shift depends upon the temperature of the source, which was unfortunately not recorded at Harwell. The experiment is now being repeated.

The Authors:

Dr. L. L. Green is a senior lecturer in physics at Liverpool. His recent publications have been concerned with γ- ray spectroscopy and the structure of nuclei in the d-shell. Dr. H. Muirhead lectures in physics at Liverpool; he is engaged at present upon research on the properties of the muon.

The Preparatory Schools Science Course at Denstone College, 1960

by S. W. HUTCHERSON

Senior Science Master, Denstone College

The Science Course held both this year and last was an attempt to assist more Preparatory Schools to introduce some Science into their curriculum and the Course in the two years was run on very similar lines. Perhaps the fact that all those taking part this year were in favour of at least some Science, whereas last year some at least of those present were doubtful of its value, may be an indication, if only a slight one, that the idea of Preparatory School Science is gaining ground.

The pamphlet produced by the Science Staff at Denstone and used at the Course was an enlarged version of that used last year, but this time an endeavour was made to cater for two groups of boys. On the one hand is the majority, the Common Entrance boys who are likely to have at least two years at their Public School before taking ' O ' level. For them we are anxious especially to arouse interest in scientific method and to inculcate the habit of accurate observation and logical conclusion from facts. On the other hand there is the smaller number of Scholarship candidates who are likely to enter the Certificate year in their Public School and without some such introduction to give them a chance of taking at least two Science subjects at ' O ' level will be unlikely, if they have an alternative, to attempt any Science at this stage.

One inevitable difficulty in suggesting a course for Scholars is that the order of selection for subject matter in the year preceding the ' O ' level year will necessarily show some variety at different Public Schools. The syllabus we have suggested is, of course, based on the Denstone scheme for the three subjects Chemistry, Physics and Biology. Of the three Chemistry is perhaps the subject least likely to vary greatly in order of treatment and it is in this case that logical sequence is most important at this stage. Biology will probably show most variety but the sequence in this case matters less.

The principles which have inspired the section on Science in the I.A.P.S. Report, ' Foundations ', seem to be excellent but we disagree somewhat in its application. It is undesirable that Science should be an alternative to any other subject at this stage while the actual syllabus content in Science subjects as suggested seems to have serious limitations. One scheme is mainly Physics with some Biology but scarcely any Chemistry. The other while lacking groundwork, includes especially in Physics, topics beyond ' O ' level standard.

The booklets on some of which the Esso practical scheme is based seem to us quite excellent as an introduction although they will hardly cover the needs of Scholars. They are attractively produced and illustrated and can hardly fail to arouse interest in the various branches of the subjects. Of the scheme itself we reserve judgement.

The Denstone Course as outlined in the pamphlet, includes suitable subject matter in all three Science subjects, with emphasis on the experimental approach. The subjects were grouped under such headings as Heat, Air, Water, Light;

with sub-divisions on the physical, chemical, and biological aspects of these main topics. It was felt to be especially helpful to those taking the Course to get as much experience as possible in the actual manipulation of apparatus and so they were encouraged to perform as many as possible of the experiments, which were listed separately in the pamphlet after the details of the subject matter itself.

To assist those with limited experience in ordering apparatus or chemicals, a list of the more likely requirements with costs was given and some suggestions were made with regard to books for general use and for a possible Preparatory Science Library.

The Course in each subject was run by the Head of that Department, Chemistry by Mr. N. M. Irvin, Physics by Mr. E. Drake, and Biology by the writer, assisted in each case by one of his colleagues. In general a short explanation of the course in each subject was given with demonstrations of some experiments and advice on others. An indication was given as to which experiments seemed suitable for use by the boys or alternatively which were better reserved for demonstration purposes. Then as time permitted the visitors were encouraged to try their skill. Film-strips were used to illustrate some aspects of the course and various films of scientific interest were shown on one of the evenings. Some variety was given by a short visit to the newly opened Science laboratory at our own Preparatory School at Smallwood.

One of the most interesting aspects of the Course was the discussion period on the last morning. This was opened by the Headmaster, Dr. Trapnell, who marshalled some of the arguments both for and against the teaching of Science in Preparatory Schools. Since the meeting was so obviously in favour he went on to consider some of the ways of dealing with the problems involved. One of the objections which have been raised to Preparatory Schools Science teaching is that of what may be called the ' virgin soil ' theory, viz. that the freshness of approach on entering a Public School at least compensated for the later start and that Science badly taught is worse than none. It has never been made clear, however, why this objection applies with particular force to Science subjects, presumably Latin and French badly taught would have the same drawbacks, and we have all heard hints dropped from time to time that not all Preparatory Schools were perfect in these respects. We see no insuperable difficulties in teaching Preparatory School Science. Anyone with a Science background at school, even if only to ' O ' level, should be capable of initiating and maintaining such work, especially we are hoping if assisted by such a Course as the one just held.

Another danger which has been mentioned of Science teaching representing too early specialization seems to have little validity. The same objection could be raised to Latin, Mathematics or any other subject. It has never been made clear why two or three periods of Science, which ought not to be an alternative to anything else, should be regarded as specialization more than five or more periods of Latin.

Cost objections have been raised from time to time, and while there is substance in this in the building or adaptation of a small laboratory, which need not necessarily be reserved entirely for scientific work, we think that maintenance costs can be kept quite low and figures are given in the pamphlet; £50 apart from the cost of a microscope should cover the equipment in apparatus and chemicals, and only a small part of this needs a regular renewal as running costs.

Objections raised with regard to teaching staff, which is a serious problem in most secondary schools, I have already attempted to deal with.

One subject on which there was some difference of opinion was that of the desirability of an optional paper in Science in the Common Entrance Exam. It was generally agreed that a compulsory paper would not, at present at least, be practicable but even an optional paper raised questions of a syllabus, which might mean restriction on the choice of material to illustrate principles. If it was to be offered as an alternative subject in Common Entrance it raised questions of assessment of relative merits of different subjects. It seemed to be generally agreed in the end that while such a paper might well be useful in the future when Preparatory School Science teaching has become more general it would not be desirable at present.

A large part of the opposition to Preparatory School Science would appear to come from the Headmasters' Conference which is a very conservative body and one on which I think it might be said scientists are very much under-represented. Change in opinion here is likely to come only very slowly.

It was pointed out, too, that it can hardly be a coincidence that relatively few Major Entrance Scholarship awards in Natural Sciences to Oxford or Cambridge go to the Independent Public Schools. It is in this field that the State Schools score so heavily. With a 4- or 5-year Science course before ' O ' level their members are vastly better equipped to undertake post Certificate and Scholarship work.

An introduction to scientific principles at an early age is essential both to broaden the outlook of all who are likely to occupy positions of leadership in later life and to give a fair chance to the abler boys to choose Science subjects if they wish—or at least to know what they are rejecting if they decide otherwise. We cannot in this scientific and technical age afford to lose the assistance of any with ability who would have made good scientists *if they had known in good time what was involved.* Nor should we train our heads of Government departments and leaders of industry without equipping them with the knowledge to estimate the merits of scientific proposals and sometimes, to appreciate the importance of speedy action, knowledge which would be more easily acquired if their early training but not necessarily their more specialized studies had included an introduction to scientific principles. A change in this respect is inevitable. It will help the cause of a full education if it can come quickly.

Research Day at the Royal Institution
The Department of Physics, Birkbeck College, University of London

by G. J. BULLEN

On May 16th, 1960, the Royal Institution held one of its " Research Days ". On these occasions a university science department is invited to demonstrate its research programme to a party of school science teachers, and the Department of Physics of Birkbeck College, University of London, was invited to show some of its work. Professor J. D. Bernal, F.R.S., head of the department, first explained the scope of the exhibits. He divided the nine topics represented into two groups, crystallography and physics proper. Professor Bernal said that he felt that research should be conducted into matters of industrial or medical interest; and much of the work shown was of that nature.

The five sections on crystallography ranged from experimental measurements and apparatus through inorganic and organic structures to the X-ray diffraction study of viruses. The exhibits in Section I (Dr. Jeffery) included a Nonius Guinier focusing powder camera, with examples of photographs taken with it; and also some Laue, rotation, and integrating Weissenberg photographs. In addition there was equipment for grinding crystals into small spheres or cylinders. The reason for doing this is that with such regularly shaped crystals it is comparatively easy to apply absorption corrections to the intensity measurements. In the next Section Dr. Mackay showed some of his work on the solid-state transformations of oxides and hydroxides of iron. It is generally difficult to obtain large crystals of the phases synthetically and efforts are made to establish correspondences between (*a*) material in a finely-divided state as prepared chemically from pure reagents and (*b*) naturally occurring minerals which may be impure but well crystallised. If only fine powders are available electron diffraction methods are used. This work bears on the study of corrosion and also helps towards understanding the mechanisms by which magnetic iron materials are produced in rocks.

A study of phase transformations in phosphonitrilic compounds formed the subject of the third Section (Dr. Bullen). Examination of several derivatives of tetrameric phosphonitrilic chloride $(PNCl_2)_4$ under the microscope has shown that they each form two or three modifications on crystallising from the melt. The modifications are stable at room temperature but, on heating to near the melting point, solid state transformations between them take place. The transitions are easily followed using a polarising microscope because the various forms have different habits and birefringences. Photomicrographs showed the course of the transformations, and there were specimens in which the processes could actually be seen taking place under the microscope.

Some of the methods for solving organic crystal structures were demonstrated by Dr. June Sutor. Dr. Finch showed X-ray work on the two different types of virus—rod-shaped and approximately spherical. For X-ray studies of tobacco mosaic virus, TMV (a typical rod-shaped virus), an oriented gel is prepared in which the virus particles have their axes parallel but lie in random orientation about these axes. A convergent monochromatic X-ray beam produced by reflexion from a bent quartz crystal is used, the cylindrical film being coincident with the focal circle of the diffracted beams. The X-ray diagram obtained is in effect the diffraction pattern of a virus particle which has been rotated about its axis, like a fibre photograph. The X-ray pattern of TMV shows that the virus protein consists of a helical arrangement of sub-units which repeats after 3 turns, the pitch being 23Å. By comparing the X-ray diagram of TMV with that of a derivative in which mercury atoms had been attached to the sub-units as marker atoms, it was deduced that there were 49 mercury atoms, and therefore 49 sub-units, in 3 turns of the helix. Comparison of the TMV diagram with that of a specimen containing no nucleic acid has shown that the nucleic acid itself lies on a helix of radius 40Å, which is tightly embedded between the protein sub-units. The small " spherical " viruses, of diameter about 300Å and molecular weight about 5 million, form true three-dimensional crystals. As the unit cells of the virus crystals are so large, very fine X-ray beams must be used to obtain sufficient resolution on the diffraction photographs. The diffracted beams are also very weak and thus very powerful sources of X-rays such as rotating-anode tubes must be used. Precession and ' still ' photographs of turnip yellow mosaic virus and polio virus were shown.

Three research topics were presented in the Physics group. Dr. Ehrenberg demonstrated the induction of conductivity in a thin film of arsenic sulphide by electron bombardment. The electrons, supplied by a $Sr_{90}-Y_{90}$ β-ray source and of average energy 1 MeV, ionize the atoms in the film and liberate valency electrons. Under the influence of an electric field applied across two thin gold electrodes, between which the film is sandwiched, the liberated electrons move towards the anode producing a secondary current which exceeds the bombarding current by a large factor, which is known to reach 40 000.

Electron bombardment conductivity gives information about the build up and decay of space charges in insulators, on carrier mobilities, life times and transit times, and details of trap levels and densities. It has applications in the detection of radioactive particles, and as a low noise beam current amplifier in television cameras, storage tubes and electron microscopes.

The second section (Dr. Jennings) comprised a determination of Planck's constant from the energy difference of electrons emitted from the K and L levels, $h\nu_K - h\nu_L$. An image of an electron source (a radioactive deposit on a fine wire) formed by a magnetic lens is inverted and also rotated through an angle which is dependent on the momentum of the electron. Measurement of this angle on the photographed image permits calculation of the momentum and hence of the energy of the electron, so that Planck's constant can be determined.

For the last exhibit, Dr. Butt showed an apparatus used for the study of the angular correlation between coincident γ-radiations emitted from excited nuclei. This correlation is dependent on the spins of the three nuclear levels involved in the cascade, together with the multipolarities of the radiations, so that its study can reveal one or more of them. The apparatus consists essentially of two NaI(Tl) scintillation counters in coincidence, the resolving time of the coincidence circuit being of the order of a few millimicroseconds. In a complicated decay scheme, it is in many cases possible to pick out the γ-rays to be studied by pulse amplitude discrimination.

After surveying these eight topics, Professor Bernal came to his own work on a theory of liquids. He explained that (in contrast to crystals) the atomic arrangement in liquids is entirely irregular, but that in simple liquids, all neighbouring atoms are approximately the same distance apart. This means that the holes between the molecules must be polyhedra with nearly equal edges. There are only five of these polyhedra small enough to have no room for another atom inside them, and as three of them cannot be fitted together regularly, the liquid structure is irregular. When a liquid is heated, the average number of neighbours decreases and the proportion of large holes increases, leading to larger volumes. Professor Bernal showed models illustrating these ideas; some of them, made of foam rubber, showed how differently shaped polyhedra could be fixed together so that they shared faces but formed an irregular conglomerate.

After tea had been served at the conclusion of the talk, the visitors were able to see the exhibits and discuss them with the members of the department.

Optical Design Research at Imperial College

An optical design group is to be set up at the Imperial College of Science and Technology, London, under the direction of Dr. C. G. Wynne, of the Wray Optical Co. Ltd., with the support of a long-term grant from the Department of Scientific and Industrial Research.

The scientific programme will be:

1. To develop new methods of lens design, including the use of electronic digital computers, and to give instruction in these methods.
2. To design and construct lens systems of orthodox type but improved performance; of novel types including systems employing aspheric surfaces; and of specialized systems required in very limited numbers but having unusual performance characteristics, such as for space research.

The design of lens systems involves very complex problems of aberration correction which are quite beyond traditional methods. In any case, full analysis cannot be done except for very simple systems for which a high degree of correction is inherently impossible. In practice, the problem consists of the reduction of up to 50 or 60 values of the aberration function at appropriate sampling points in the aperture and field, these values being derivable numerically and depending on up to 50 design parameters.

Dr. Wynne's work has hitherto been done with the Wray Optical Company, with a Ministry of Aviation contract. The Research Council consider that this grant will enable him to develop his ideas more freely. It will also supplement the facilities already available to the British optical industry for advanced training in lens design.

Dr. Wynne was the honorary business secretary of the Physical Society, and is the honorary secretary of the new joint body, The Institute of Physics and The Physical Society. He has been English language editor of *Optica Acta* since 1954.

Contemporary Reading

The book reviewing service of this Journal is planned to a system. Advanced and specialist books are being placed in most excellent hands. A number of the more elementary or generally interesting ones have so far been dealt with editorially, either brazenly signed or timidly anonymous. It seems tidier to make a proper job of this, and to group together all those that one would personally recommend to a colleague for his own reading or for the library. The notices will be short, but the mere inclusion of a book in this feature is an indication that it is considered very good; there will be criticism, of course, but only related to these terms.

First, Prof. NORMAN FEATHER'S *An Introduction to the Physics of Mass, Length, and Time* (Edinburgh: University Press, 18*s*.) deals with what is more or less the traditional A-level ' Properties of Matter ' work in a quite untraditional way. About half the book covers the basic concepts of mechanics and their establishment, and the physical principles behind the prescription and reproduction of standards. Gravitation (with a good deal of history and astronomy) and surface tension (in terms of interfacial energy) are very well done. The simple mathematics and deliberate rate of progress recall the masterpieces of Cox and Perry; perhaps it would be better to say that Prof. Feather has revived an old, but lapsed, tradition of writing for people who want to understand rather than to exam-cram. It is good to see elementary work handled with so sure and scholarly a touch, and presented with a happy literary style.

Of *Selected Lectures in Modern Physics for School Science Teachers*, edited by Prof. H. MESSEL (Macmillan, 30*s*.) I can only repeat the praise that I wrote elsewhere two years ago. The lectures were given at the first Summer School sponsored by the Nuclear Research Foundation at the University of Sydney in 1958. The stature of this enterprise can be gauged by the fact that the lectures were printed, more or less in the present form, for issue at the School, and circulated on a limited scale at the time.

The topics covered range from ultra-high-energy nuclear physics and the Australian work on radio-astronomy, to Newton's laws of motion applied to rocketry, and the concept of temperature. The distinguished contributors include H. MESSEL, D. F. MARTYN, E. W. TITTERTON, and SIR MARCUS OLIPHANT. Editor and publishers must be congratulated on the finish and production of this edition. Its successor, *From Nucleus to Universe* is reviewed separately; can we hope in due course for a Macmillan edition of this?

The 1958 Royal Institution Christmas Lectures are published under the title *The World Around Us*, edited by SIR GRAHAM SUTTON (English Universities Press, 18*s*.). They record various aspects of the International Geophysical Year investigations. J. A. RATCLIFFE deals with electron-density studies of the ionosphere; J. M. STAGG with the earth's magnetism; and R. L. F. BOYD writes on the exploration of the upper atmosphere by satellites, and their tracking and instrumentation. SIR GRAHAM SUTTON discusses the lower atmosphere and its weather, and G. E. R. DEACON describes oceanographic research on the sea and its problems. The work undertaken in the Antarctic, and the problems of establishing and maintaining an Antarctic base, are described by G. DE Q. ROBIN. An excellent book, very well produced and illustrated, which will appeal to the general reader as well as to the scientist.

DESMOND KING-HELE's *Satellites and Scientific Research* would probably have met with the essay-review it deserves, but the author will be contributing an article on the subject to a later issue. In the early chapters he deals with the life-history of a satellite, from launching to re-entry, and gives data for all the successfully orbited ones so far. The methods of tracking are described, from the surprisingly effective visual observations by volunteers and by the kinetheodolite, to the use of radio interferometers and the Doppler effect, and the radar-echo observations at Jodrell Bank. As results already obtained, we are told of the shape of the earth, the measurement of the polar flattening and its asymmetry from orbital-plane rotation, the exploration of the earth's gravitational field, the upper atmosphere and the difference between what was found (on first looking into Chapman's corona) and expectation, in the way of density, composition, and temperature, and the radiation belts. The author writes clear, compelling prose, and quotes poetry appropriately. A first-rate book for sixth-form reading, and very good value. It is published by Routledge and Kegan Paul at 21*s*.

Mr. John Lewis mentioned the Physical Science Study series of readers, published in the U.S.A. for the P.S.S.C. by Doubleday-Anchor, in his article last February, and one of these, DONALD HUGHES' *The Neutron Story* was reviewed by Sir James Chadwick in our December issue. It is good news to learn that they are to be published in this country by Messrs. Heinemann in October, 1960, at a price of about four shillings. The others before me now are: a reprint of VERNON BOYS' *Soap Bubbles*; *The Birth of a New Physics*, by BERNARD COHEN, which deals with Newton's work on gravitation; *How Old is the Earth*? by PATRICK HURLEY; *Waves and the Ear*, by W. VAN BERGEIJK, J. R. PIERCE, and E. E. DAVID; *Echoes of Bats and Men*, by DONALD GRIFFIN (may its authentic title appeal to schoolboys!); *The Physics of Television*, by D. G. FINK and D. M. LUTYENS; the autobiographical *Magnets*, by FRANCIS BITTER; and a superb larger one (which will cost more) *Crystals and Crystal Growing*, by A. HOLDEN and P. SINGER, illustrated in colour. These are readers, not textbooks, designed to supplement a basic course in physics. They should fulfil their purpose admirably. It is to be hoped that, in this respect at least, the example of P.S.S.C. will be followed in Britain. If people are conditioned to read only advertisements, examination questions, and textbooks (in that order of precedence) what do we really expect in the way of literacy? Good, inspiring science books, and floods of them, at a price people can really afford—a saturation operation, not just a few driblets—would help to establish the scientific *culture* that is needed.

From THE MINISTRY OF EDUCATION comes an excellent publication that all science teachers will welcome—Pamphlet No. 38, on *Science in Secondary Schools* (H.M.S.O., 4*s*. 6*d*.). This is an enlightened and stimulating document, which deals realistically with the problems in schools as they are today. I learned a lot from it about the presentation of physics at various stages, found a number of ideas for experiments, and was led to think carefully again about the logical treatment of electricity in modern terms. I am not sure that I agree with the recommendation that electrostatics should be deferred till after 15—the subject is based on the conservation of energy and the conservation of charge, and pupils have got to appreciate both concepts early; nor do I despise the tangent galvanometer, which can be converted without much effort into a plane circular coil; but the ‘ voltmeter and *J* ’ is a matter of integrity that just has to be faced. On organization of courses, the Pamphlet gives sound advice. The whole aim of science teaching in schools, to encourage people to think scientifically instead of bowing to ‘ the tyranny of the right answer ’ is emphasized throughout. As to present possibilities of implementation, the booklet *Science Teaching in Secondary Modern Schools*, a survey of conditions and staffing carried out by the Science Masters' Association Secondary Modern Schools' Committee and published in May, 1960 (price 1*s*.), gives up-to-date facts. But the Pamphlet does not gloss over this situation and does, in its observations on the training of teachers, mention some steps to deal with it.

With its May 1960 issue, *The Advancement of Science*, Vol. XVII, No. 65, (7*s*. 6*d*.), published by the British Association, appears in a new and most attractive format that heralds its widened scope as a medium for diffusing scientific knowledge. In future it will appear six times a year, and contain contributed articles from leading scientists as well as some papers given at Meetings. Among the ten articles are *Teaching Science to Juniors by the Discovery Method*, by N. F. NEWBURY; *Problems of the Moon*, by PATRICK MOORE, and *Some Aspects of Crystal Physics*, Part I, Electrons in Crystals, by Prof. G. F. J. GARLICK, and Part II, Coloured Crystals as Model Solids, by R. V. HESKETH and E. E. SCHNEIDER, admirably clear and strikingly illustrated. I ought to list the full contents, since *all* the articles on other branches of science as well will be of interest to physicists, and are of the same excellent standard. But there is no reason why I should reveal more, when you ought to support this devoted work on your behalf by taking out a subscription at once. The Association's address is 18, Adam Street, Adelphi, London, W.C.2.

Finally, two publications that have just come to hand as this article is being written, which will be reviewed more fully later. The first is *The National Physical Laboratory Report for* 1959 (H.M.S.O., 8*s*.), a handsome production. The second is *A Physics Anthology*, edited by NORMAN CLARKE (Chapman and Hall, 35*s*.), in which Mr. Clarke, the Deputy Secretary of the Institute of Physics and Editor of the Institute's *Bulletin*, has collated a number of articles that were first published in that Journal.

G. R. NOAKES.

ESSAY REVIEW

by SIR ARNOLD PLANT

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Electronic Computers and their Business Applications. By A. J. BURTON and R. G. MILLS, with a foreword by A. J. BARNARD. (London: Ernest Benn Ltd., 1960.) [Pp. xv + 325.] 45*s*.

These are still early days for a full appreciation of the business potentialities of electronic computers and of the conditions for their profitable use. Rapid progress awaits the appearance in much greater number of two types of collaborator; on the one hand, business leaders with the flexibility of mind, courage and drive to master the basic technology and face the probable necessity to scrap and re-design the systems of record and communication at present in use in large organisations, and on the other hand, electronic specialists who are determined to understand the problems facing administrators in a changing world and invent systems and adaptations of machines which will yield significant economies and open up avenues towards entrepreneurial activities not hitherto attainable. There is almost certainly scope for a new type of consultant who can earn the confidence of his business clients and draw up the specifications to which machine producers can be induced to conform. Component elements will achieve standardization before complete installations, if progress is to be rapid in giving full satisfaction to clients with diverse needs.

This book aims to introduce business men to the basic technology, and to provide electronic specialists with information about business applications. The first need is probably the more urgent, particularly at company board and senior executive level, where decisions on heavy capital investment and major policy are taken. The second is best tackled at a lower level, presenting the facts about the routine of working out systems, ascertaining requirements and surveying feasibilities, for the benefit of junior mathematicians and technologists who need to be instructed about business processes, rather than for computer experts who are likely to be well briefed by their selling organizations. For these two purposes the authors have planned their work quite well.

A dozen concerns are already producing a variety of computer equipment for sale in Britain, and the two authors had first to decide how best to present their explanation of the basic scientific ideas and methods of working involved in these machines. As lecturers on the staff of the Sir John Cass Institute in London, they have chosen to build their exposition on a hypothetical drawing-board model computer, which they call a CASSEAC, incorporating the main features to be found in marketed machines. The data are coded on the binary system, information being introduced through four input channels, two using magnetic tape, the others for punched card or paper tape; and the computed output is delivered through four similar output channels, with direct printing. The computer stores the programmes (or instructions to the machine) as well as data, in two accumulators, one working store giving quick access to a relatively small reservoir, using a magnetic core, and a larger but slower working auxiliary store of the magnetic drum type. In the first section of the book, three chapters are devoted to explaining the binary code system of translating words and decimal numbers into machine data, and the way the computer works in processing data, obeying instructions which make up the programme to be followed and indicate the address or location in the store of information to be extracted. The exposition seemed to me to be adequate, although hampered by a reference on p. 45 to ‘ the table below ’ which presumably should have directed me to p. 44.

The ten chapters comprising Part II explain the techniques for developing projects for using computers in business applications. Following a 1958 report issued by the Institute of Chartered Accountants, which defines electronic data processing as ‘ analysing, marshalling, recording and reporting business information ’ by means of digital computer equipment, the account continually stresses the importance of starting from the definition of business aims hitherto unattainable and examining how they might be realized by using the special facilities offered by computers, rather than from the idea that existing office procedures might be more efficiently conducted with their aid. This expensive equipment depends for its profit-earning potentiality on improvements in business procedures rather than on savings accruing in the transfer of existing routines to computers. The chapters on programming accordingly explain in sequence the processes by which experts on business systems should first prepare complete job specifications, from which programme staffs can work out the detailed flow charts. Much of this preliminary work can be planned, in consultation with computer manufacturers, in advance of final decisions concerning the particular equipment to be installed. Thereafter the flow charts must be developed into detailed coded programmes, allocating positions in the accumulators to all the data, embodying the processes in correct instructions, and preparing the written sequences of operations for the guidance of junior operatives. These chapters are well designed, and deserve special commendation for the frank emphasis on problems which have invariably to be faced and solved, if computer installations are to be fully successful in normal operation.

Part III of the book describes actual or planned business applications of computers, in Britain and the United States. The first example is payroll, in which the complications of P.A.Y.E., holiday payments, piecework, bonuses, and insurance contributions offer clear opportunities for more speedy handling of pay sheets, individual employee records and periodical statements. Then comes sales accounting and stock control, a particularly rich field for application because of the important development made possible in attaining the optimal control over the maintenance, inflow and outflow of dispersed depot stock-holdings. Local government applications to rate accounting, and to itemized expenditure against budget allocations, form the subject-matter of a special chapter contributed by Mr. J. W. Cox. For an example of use in the field of life insurance business, in maintaining a complete register of policy-holders, the authors had to turn to the United States. Already, however, while this review is being written, two British insurance offices have announced the creation of a subsidiary company in Edinburgh to share a computer for the initial work of recosting and valuation of group pension schemes, utilizing paper tape, punched card and magnetic tape for the input and output channels, and equipped with an on-line printer. In banking, the application of computers to central clearing houses and to the daily book-keeping of current accounts is described. Here again it has recently been announced that one of the English clearing banks has operated a computer on current account book-keeping in a London branch since 1 January 1960 and has a machine on order to extend the system, including the production of statements of customers' accounts, to cover the work of several branches. Finally, the field for application of computers to Stock Exchange dealings and to the maintenance of share registers is discussed. An appendix contains photographs of representative machines.

The authors deserve thanks for the valuable service they have rendered in compiling this, on the whole, intelligible account of a highly complex but most important technological development. The exposition can, and in subsequent editions no doubt will, be clarified and improved. Serious readers would be well-advised, between reading the book for the first and second time, to make a detailed study-visit, under expert guidance, to a concern which has an established computer installation at work.

The present reviewer has gained a great deal from opportunities to do this, and in particular from an intensive study of the large installation in operation at the U.S. Army signals establishment in Philadelphia. Without that advantage he would have found this book heavy going.

BOOK REVIEWS

Die Fernrohre und Entfernungsmesser. By A. KONIG and H. KOHLER. 3rd Edition. (Berlin: Springer-Verlag, 1959.) [Pp. 475 + viii.] 88 *dm*.

This is the third edition of the well-known book on telescopes and range finders, which was first published in 1922. While there are at present available numerous works on optics treated as a whole subject, and short monographs on particular branches of technical optics, this volume treats in a thorough manner the whole field of telescopes and related instruments from both the scientific and technological points of view. The book will obviously remain a major work of reference for years to come. One interesting feature is the inclusion of a final chapter on the history of telescopes which stresses the remarkable rate of technological development of telescopes two, three, or even four centuries ago.

The references to literature are up to date (1959) and full and the book is well-produced and copiously illustrated. Every part of the subject appears to be treated, and on the scientific side emphasis is placed on recent developments in the wave theory of aberrations and on aperture considerations. On the technological side accounts of interference effects in thin films are treated.

C. A. HOGARTH.

Electronic Contacts Handbook. By RAGNAR HOLM, aided by ELSE HOLM. 3rd Edition. (Berlin: Springer-Verlag, 1958.) [Pp. 522.] 52 *dm*.

It is now generally recognized that the apparently simple operation of an opening switch or contact in an electrical circuit can involve a large variety of phenomena covering many branches of physics. When the operation is on a large scale, as in a contact breaker of a high power circuit, the phenomena are so obvious that attention is readily attracted to them. On the other hand, in low power circuits, as for example when a current of about an ampere is interrupted in a low voltage circuit of about a volt or so, then the interesting phenomena occur on a micro-scale which require special methods of investigation. Indeed, in such circuits, as well as in the so-called electrostatic circuits with currents of less than a micro-amp and potentials less than a micro-volt, it may not be obvious that anything in particular occurs at the opening contact. In fact, the physical investigation, particularly that since World War II, has shown that the processes occurring involve fundamental mechanisms of electron emission from cold conductors and semi-conductors, of boiling metal, of electrical and thermo-electrical processes in molten metals, of gas discharge and ionic transport phenomena on a micro-scale, as well as non-electrical problems of friction and wear, powder metallurgy and welding. Thus, the full treatment of electrical contact phenomena will be of interest to metallurgists and engineers as well as to physicists.

The Electric Contacts Handbook by Ragnar Holm attempts to cover the whole field in 522 pages. Dr. Holm is known as the author of *Electrical Contacts*, published in 1946 and based on his earlier *Technical Physics of Electrical Contacts*, and the present volume appears to cover much the same ground except that it has some references to work carried out since the earlier books were published ; in fact, the present work can be regarded as a third edition of the first book. As the subject is so wide, and the range of application so extensive, treatment of many important aspects can only be cursory, and references are given in place of detailed consideration of the fundamental processes. The crowding of so much into this survey is, perhaps understandably, done at the expense of providing a clear critical assessment of the scientific position regarding present knowledge of the fundamental processes involved.

A handbook which attempts to cover so much ground is open to the objection that the reader is not given a critical analysis of the present situation. This book does not repeat all the pre-war experimental results on questions such as mass transfer which was contained in the author's previous volume. Although this book gives an account of some practical arc-suppressing circuits and some physical data on common contact metals, the treatments of the basic physics of the initiation and maintenance of the arc, the development of the microscopic metal bridge and its significance, are the least satisfactory parts of the book. The reader should be careful in using the numerous tables giving experimental data, particularly those on matter transfer. Indeed, it would have been helpful if the book had contained a more critical assessment of the validity of much of this data especially when some of this is attributed to bridge transfer rather than arc. The value of such data is doubtful unless the experimental conditions were controlled and known to be controlled. The book is likely to be helpful to those who are interested mostly in the technical aspects of the arc and who wish to be given an introduction to some of the fundamental ramifications involved, but does not absolve the reader from consulting the original papers.

F. LLEWELLYN JONES.

From Nucleus to Universe. A Course of Selected Lectures in Physics and Astronomy in the University of Sydney, January 11–22, 1960. Edited by PROF. S. T. BUTLER and PROF. H. MESSEL. (Sydney : Shakespeare Head Press Pty. Ltd., 1960.) [Pp. 335.] 84*s*.

The first of these excellent lectures, by Prof. S. T. Butler, deals with nuclear structure and the interaction between charged particles and radiation. Next, Prof. C. N. Watson-Munro discusses the physical conditions for maintaining a thermonuclear reaction, the problems of stability in both linear and toroidal pinch machines, the field convolutions of the Stellerator type, and the magnetic-bottle devices.

Prof. G. Gamow discusses both the evolutionary and steady-state cosmologies, together with the kind of evidence which would help to discriminate between them.

R. Q. Twiss describes the Michelson interferometer as originally used for the measurement of stellar diameters, and then shows how, having been converted into an *intensity* interferometer for radio work, it has come back as an intensity interferometer into optics.

T. Gold discusses the origin of the solar system, the structure and evolution of the earth, and the probable state of the surface of the moon. A. A. Day mentions the information expected to be given by the project of boring a hole down to the Mohorovičić-discontinuity. D. F. Martyn deals with the problems of launching satellites, the processes of observing and tracking and obtaining information from them, and the results of satellite observations. H. D. Rathgeber describes the Van Allen belt of radiation, and its probable part in the origin of the aurorae. G. B. A. McCusker gives an account of cosmic radiation, its association with solar flares, and some of the very high-energy nuclear events observed in photographic emulsions exposed to cosmic rays. The study of the upper atmosphere by observations of meteor trails, micrometeorites and the zodiacal light, is treated by G. Elford. Finally, K. B. Mather writes of research in Antarctica during the International Geophysical Year, and of work on the structure of the Antarctic continent.

If we are to begin to think seriously, along the lines suggested by numerous recent reports and publications, about science as a liberal study in schools, this is the kind of book that can provide some valuable guidance.

G. R. NOAKES.

Gas Calorimetry. By C. G. HYDE and M. W. JONES. (2nd Edition.) (London : Ernest Benn Ltd., 1960.) [Pp. 456.] 105*s*.

This work, which first appeared in 1932 and has been a standard reference book on all matters relating to the determination of the calorific value of gaseous fuels, is now published in an entirely revised and enlarged second edition.

If the calorific value of a gaseous fuel is to be determined accurately and intelligently, a complete understanding of the instrument, conditions of measurement and possible sources of error, must be the aim of the operator. The authors, Major C. G. Hyde and Mr. M. W. Jones, have obviously made this their objective and, with a wealth of experience, have produced a book which, with its easily identifiable sections, its lucid descriptions of various types of instrument and chapters devoted to accommodation and equipment, water supply and measurement, temperature measurement and other important items, must become a necessity on the bookshelf of every fuel engineer. All the recent developments have been included and appendixes, eleven in all, are a most useful and important feature.

The book is marked by the same high standard of detailed accuracy and clarity of presentation as its predecessor, is lavishly illustrated with plates and diagrams, and is carefully indexed.

E. J. GRAYSON.

The Theory and Design of Inductance Coils. By V. G. Welsby. 2nd Edition. (London: Macdonald, 1960.) [Pp. 232+xviii.] 30*s*.

The first edition of this book appeared in 1950 and it has now been considerably revised and expanded. The main theme is the design of inductance coils with air, laminated iron, dust-iron or ferrite cores. By means of formulae and charts the properties of such coils can be computed with a useful degree of accuracy.

Almost more space is given to the secondary properties of self-capacitance and Q than to the main attribute of inductance. This is as it should be for, in practical applications, it is these properties which usually govern the kind of coil which must be used. The required inductance can often be obtained with almost any form of construction; especially at radio frequencies, it is the limitations on tolerable self-capacitance and the necessity for a certain minimum Q value which govern the choice of wire, the form of winding and the kind of core which must be used.

The book is an excellent one in that it deals clearly with all these matters. If it is read and understood, it should enable anyone to design coils with reasonable success. Design is not an automatic procedure, however, and it must not be thought that one can obtain constructional data for a coil just by inserting required figures into formulae and doing a few sums.

This may occasionally happen in very simple cases but, all too often, one approaches a final design only through a series of tentative designs. If the initial specification is a ‘ difficult ’ one, even an experienced designer may be taxed to meet it and will be involved in a lot of hard work.

The book is one which can be thoroughly recommended and contains a great deal of information which is not readily available elsewhere. The reviewer knows of no other single book which includes such a wide range of information on coil design. Few coil designers need look outside it, save for information about the characteristics of specific core materials. That is not included to any great extent and rightly so, for there are so many different materials and they change so quickly. A bibliography is included for those who do want more detailed information on specific points. W. T. Cocking.

Report of the Research Council of the Department of Scientific and Industrial Research for the year, 1958-59. (Cmnd. 1049, H.M.S.O., May 1960.) 4*s*. 6*d*.

During 1957-58, 119 grants for research, totalling £445 000 were made by D.S.I.R. In 1958-59, the 295 grants amounted to £2 232 000. These figures measure the expansion and acceleration of the Council's enterprise. The Report mentions also two other features, changes in the functions and organization of some of the Department's research stations, and the development of cooperation between the various research organizations.

Of the D.S.I.R. research stations, the Radio Research Station at Slough features prominently. Already noted for its contribution to the International Geophysical year, and more recently as one of only three permanent World Data Centres, it will devote one half of its total effort to space research. Using rocket and artificial satellite techniques, the Station will join with U.S.A. and Canada in a project for sounding the ionosphere *from above*. Much of our present knowledge of the ionosphere is derived from observations of radio waves reflected from below; this will be extended by observing reflections from the satellite transmitter back to the satellite, the information being transmitted over observatories at Slough and Singapore, and possibly in the Falkland Islands and elsewhere.

The University research projects are listed in Appendix VII, which gives a comprehensive directory of what is going on. Major developments include studies of arch dams of the Kariba type, the acquisition of a 10 Mev tandem Van de Graaff for Liverpool, further instalments to Jodrell Bank, and a grant to Prof. M. Ryle at Cambridge for radio-astronomy, the application of the electron-transmission microscope to metallurgical studies at Cambridge, and work on geophysics (Durham) and underwater geology (Bristol).

In addition to these projects, the Council also deals with several types of postgraduate training award, including the Senior Visiting Fellowship scheme launched under O.E.E.C. The number of research fellowships was three times that of the previous year; research studentships also increased rather more than had been expected. The new Advanced

Course studentships have expanded in numbers, but rather disappointingly, and consideration is being given to the most useful way of distributing them. The special scheme for studentships and fellowships administered in this country on behalf of N.A.T.O. has started strongly; 18 studentships and 22 fellowships were awarded, most of them for study abroad.

Mechanical Radiation. By R. B. LINDSAY. (New York: McGraw Hill Book Co.) 77*s*. 6*d*.

In all but the most elementary of physics courses, a student soon finds the need for some understanding of waves (usually assumed to be harmonic) and wave propagation. It is Professor Lindsay's thesis that it is advantageous to introduce the subject by considering mechanical radiation, since the student may then more readily grasp what is meant by a wave. This may well be true; certainly in the first hundred pages of this book the student is presented with an excellent introduction to the subject, covering in general terms the elementary properties of harmonic waves and damped harmonic waves, reflexion, refraction, diffraction and interference and, finally, some effects of imposing boundary conditions.

The subject matter then becomes rather more specialized, dealing with the wave motions often considered in advanced textbooks on sound, with elastic waves in the atmosphere, with seismic waves, and finally with a variety of wave propagation problems in solids and liquids. The presentation throughout is most concise, with the physical principles of each topic clearly indicated.

There is probably more in this volume than the average student will require, but he can gain from it an excellent understanding of the subject. K. J. STANDLEY.

A French Course for Technologists and Scientists. By H. SHELDON JACKSON and J. STANDING. (London: Harrap.) 16*s*.

This is a most helpful and useful book, destined to serve admirably its purposes as a translation book for adult beginners, and a refresher course for those who have dropped language study for some time to specialize in science. It may well help Modern Language teachers in schools to persuade their scientific colleagues of the need for more periods of French for science students!

There is a succinct and realistic Grammar section of some 32 pages in which the examples are well-selected and, wherever possible, firmly based in the scientific idiom. A hundred Sentences for Translation follow, which to be of maximum use to the individual student might well have been accompanied by model translations.

The main section of the book consists of 150 Passages for Translation (of which the first 17 are introductory) with an immensely wide range of subject-matter. These passages are classified under Mechanical Engineering, Electrical Engineering, Civil Engineering and Chemistry. The choice of passages is commendable and their value even to a non-scientist. is self-evident; they are full of remarkably interesting ' tit-bits ' of information, and from a linguistic point of view the comparison of French ' controler ' and English ' control '—by way of ' le self-control '—is particularly fascinating (pp. 75, 76).

One addition which might have been made is a number of simple diagrams, which, well-labelled, would have done much to help in the mastering of French technical terms in the field of instruments and equipment.

This useful manual is well rounded off by suggestions for further reading, a good and accurate vocabulary, and an invaluable list of scientific abbreviations. D. A. EMMS.

Optics (Vol. 3 of A Degree Physics). By C. J. SMITH. (Arnold, Jan. 1960.) [Pp. vii + 736.] 63*s*.

This book deals with the usual course in undergraduate optics. Successive chapters discuss, in considerable detail, the image forming properties of the single surface, the thin lens, coaxial systems of thin lenses, and the thick lens. A weakness of this section of the book is that it is not made sufficiently clear that, when suitably expressed, the image forming properties are the same for all optical systems, however complex. Here, as elsewhere in the book, the information is given but the point is not clearly made. In a book for students this latter is a serious criticism.

Following the chapters on lens theory, there are sections on photometry, colour, interference, diffraction, and polarization. One very welcome feature is that the book contains a large number of examination questions, including worked examples, particularly in the geometrical optics sections.

Nobody expects a book to be completely free from errors, but this book does contain a rather large number and some of them are particularly unfortunate in a students' text because they are not detectable by a reader without previous knowledge. One example, is the attempt to show a variation of distortion with stop position for a thin lens for which the first four Seidel aberrations are zero. Now, if the lens is thin and the first four aberrations are zero, there is no distortion, and even if the lens is not thin the distortion will be independent of the stop position. However, it is unlikely that the student will know this.

Other errors include the popular mis-statement that for Fraunhofer diffraction patterns the wavefronts incident on the diffracting aperture must be plane, and the equally common " proof " that with a point source the fringes of equal thickness formed with a thin wedge film are localized—in fact they are not. The author does not explain why it is essential to employ a cylindrical lens with a Rayleigh refractometer in order to employ the fiducial fringes, and it is implied that one can use a cross-wire with a cylindrical lens.

It is good to find an up-to-date account of methods of determining the speed of light, but on the other hand it is almost unbelievable that in a book published in 1960 there should be the statement that for " general photographic purposes the achromatism is effected with respect to the mean D line and a line in the extreme violet, because the eye is most sensitive to yellow light while the plate is coated with an emulsion most sensitive to the so-called actinic rays". Equally surprising is that when discussing the use of diffraction gratings it is stated that " in modern spectroscopy . . . a photographic method is always used ".

Other unfortunate statements that a student is likely to accept include ". . . it is a well-known fact that vision in a dim light improves after a time; this is due to dilation of the pupil" and "The extreme ultraviolet region extends from 1850 Å to about 150 Å, which is a region of great opacity for all known solids with the exception of fluorite which is transparent down to about 1200 Å ".

Less serious are those errors that can be detected without previous knowledge—as, for example, in several diagrams.

A useful feature of the book is that it includes a number of things for which space is not usually found. These include the transverse axis and the self-conjugate points of a coaxial lens system and the Fresnel three-mirror interferometer. On the other hand it is surprising that in a book of over 700 pages on degree level optics there is no mention of Maxwell's equations.

R. S. LONGHURST.

The Hollow Universe. By CHARLES DE KONINCK. (Oxford University Press, 1960.) [Pp. 127.] 12*s.* 6*d.*

" Is the hollow in the universe or in our heads ? " Dr. de Koninck's answer to this question, posed by a small girl who saw the title of this book, is contained in three studies of the approach of modern science, through mathematics, physics, and biology, to the universe. His thesis is that " the absence of coordination between the sciences and the failure of each to reflect upon the scope and significance of the others, have brought all of them to a state of hollowness and shapelessness like the grin without the cat ". The hollow is then the hollow in the scientists' philosophy. Instead of reflecting on his own nature in its wholeness, the scientist is content to picture his mind as a machine built on the model of a computer, his body as a bundle of physico-chemical events, and his " life " as meaningless because it is meaningless to ask " what is life ? "

These three lectures given to an undergraduate audience at McMaster University, Toronto, provide a masterly introduction to the dialogue between the old philosophy of nature based on the Aristotelian method and the philosophy of nature which is implicit in our " sciences of nature ". The failure of these sciences to converge upon the single, though infinitely varied, whole which is their object, has led the scientist into being far more ignorant of man than Sophocles or Shakespeare. This argument for wholeness versus hollowness makes *The Hollow Universe* fascinating reading.

M. L. HUGHES.

The Metallic State

by A. H. COTTRELL

Goldsmiths' Professor of Metallurgy in the University of Cambridge

SUMMARY

Metals contain free electrons. Various theoretical and practical implications of this characteristic feature of the metallic state are discussed. Recent views which emphasize ease of ionization, unsaturated metallic bonds, and screening of electrical charges by free electrons, as the basis for distinguishing between metals, semi-conductors, and insulators, are described. The importance of the free electron bond for the crystal structures, cohesive properties, and alloy behaviour of metals is emphasized, and the principles underlying the engineering strength properties of matter and the design of creep-resistant alloys are briefly discussed in terms of free electrons and dislocations in solids.

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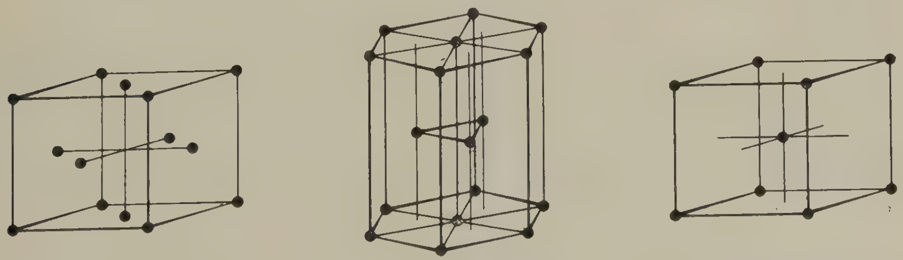
1. INTRODUCTION

The properties of metals such as mechanical strength, ductility, electrical conductivity, and ability to combine in various proportions to form alloys, are so remarkable and so valuable to mankind that a special branch of physical science has grown up to deal with them. It is the task of the metallurgist to develop these and other properties to greatest effect in practical materials by placing the right atoms together in the right amounts and in the right positions. Great changes have taken place in metallurgical science in recent years and, as a result of our much better understanding of the metallic state, it is becoming possible to design metals and alloys systematically from a knowledge of atomic properties. The constant interplay of pure and applied science provides one of the fascinations of modern physical metallurgy.

An ordinary piece of metal usually consists of many small polyhedral crystal grains completely joined together by grain boundaries, these being transition regions two or three atoms thick across which the crystal lattice changes its orientation from one grain to the next. It is general practice to make the grains small, e.g. about 0·1 mm across, by various alloying, casting, working, and

Fig. 1. Crystal Structures of Metals.



Face-centred Cubic (Cu, Ni, Al, Pb, Fe at high temperatures)

Close-packed Hexagonal (Mg, Zn, Cd, Be)

Body-centred Cubic (Li, Na, K, W, Cr, Mo, V, Nb, Fe at low temperatures) other treatments, because a small grain size gives good mechanical properties, but it is not difficult to grow large single crystals when necessary. The atoms in metal crystals are usually arranged in strikingly simple patterns, such as may be produced by stacking equal spheres together in close-packed formations. Figure 1 shows the three most common crystal structures, with the atomic spheres contracted to points for clarity.

The close packing of the atoms in these structures is indicated by the high ' co-ordination number ', the number of nearest neighbours to an atom. In a face-centred cubic metal, for instance, this is twelve. Such high co-ordination numbers obviously cannot be explained by ordinary notions of chemical valency. The well-known rule that an atom of valency N can bond with $(8-N)$ neighbours, which works so well in, for example, tetrahedrally bonded diamond, clearly breaks down completely when applied to metals.

2. Free electrons in metals

Metals differ from non-metals most strikingly in their electrical conductivities, those of copper, silver, and aluminium, for example, being 10^{20} times larger than those of diamond, mica, and aluminium oxide. Drude (1902) and Lorentz (1916) explained this by postulating the existence in metals of ' free electrons ', that had become dissociated from particular atoms in the metal and moved freely as a kind of gas or ' plasma ' through the entire network of atoms. This idea has dominated the whole subject ever since. As we shall see later, however, it is not the ability to conduct electricity by the passage of free electrons that distinguishes metals from insulators, for the same process can be made to occur in an insulator. The special feature of a metal is that electrons in it remain free right down to the lowest possible temperatures. In fact, the electrical conductivity of a metal improves on cooling to low temperatures whereas that of an insulator or ' semi-conductor ' decreases towards zero.

The Drude-Lorentz theory has been extremely successful. Electrical and thermal conductivities, and optical properties, were explained in terms of the movements of free electrons in electrical and thermal gradients. Tolman and Stewart (1917) proved that sudden pulses of current could be set up by shaking metals sharply, so that the free electrons were thrown towards one end.

Measurements, particularly of electrical conductivity, showed that in a metal such as silver there is approximately one free electron per atom. Considered as a gas, the electronic fluid is thus extremely dense and it is not surprising to find that it behaves in a radically different manner from other gases. Sommerfeld (1928) showed that this high density leads to important quantum effects, particularly on the thermal properties of the electron gas. To describe a gas by the classical kinetic theory it must be physically possible, in principle at least, for us to separate individual particles of the gas. If n is the number of free electrons per unit volume then we must be able to locate the position of any electron with an error $(\Delta x)^3$ smaller than n^{-1}. But we know from Heisenberg's uncertainty relation that a particle cannot be located with a precision Δx unless its momentum exceeds a minimum value Δp given by

$$\Delta p \, \Delta x \simeq \boldsymbol{h}, \tag{1}$$

where $\boldsymbol{h}$ is Planck's constant ($6{\cdot}627 \times 10^{-27}$ erg. sec.). Since the energy E is related to momentum p and mass m through $E = p^2/2m$, this means that the gas cannot behave according to classical kinetic theory until it has been raised to such a temperature that the thermal energy per electron exceeds the value

$$E_0 \simeq (\Delta p)^2/2m \simeq h^2 n^{2/3}/2m. \tag{2}$$

The exact result is $E_0 = (h^2/8m)\,(3n/\pi)^{2/3}$, which corresponds to a temperature of 60 000° K for silver. At ordinary temperatures the motion of the electrons is completely dominated by quantum effects. Even at 0° K the electrons are in motion with speeds of order 10^7 cm. sec.$^{-1}$ and they occupy a band of quantized kinetic energy levels up to a maximum at E_0. This state of affairs persists almost unaltered at all temperatures up to the boiling point.

We must now ask why some substances are insulators and others metals. There are two approaches to this problem. The older one takes the existence of free electrons as a starting assumption and then sets out to prove that under some circumstances they can transport a current and under others they cannot. This theory has the advantage of being suitable for detailed mathematical treatment, but, as Mott (1958) has pointed out, it predicts to be metals some substances that are known in fact to be insulators. It would seem to have chosen not quite the right criterion for deciding between metals and insulators. The other and more recent approach starts with insulators and considers the conditions under which their electrons may become free. We shall follow this second approach, believing it to be intrinsically sound even though it is not yet capable of exact mathematical treatment and provides us with only a blunt-edged criterion.

3. Ionization

Matter is normally electrically neutral. An electron of charge $-e$ is attracted to a proton of charge $+e$ at a distance r by a Coulomb force e^2/r^2, and the potential energy $V(r)$ of the electron in the field of the proton is given by

$$V(r) = -e^2/r. \tag{3}$$

A neutral hydrogen atom exists when the electron is captured in this field and moves in a ‘ bound quantum state ’ around the proton. Ionization into mobile, free, charged particles is essential for electrical conductivity. This occurs most obviously in hot gases, when the temperature T(°K) becomes sufficient to ionize a large number N of the atoms or molecules, according to the formula

$$N = \text{const.}\ \exp\{-I/2kT\}, \tag{4}$$

where I is the ionization energy and k is Boltzmann's constant. At low temperatures the gas is an insulator, then it behaves like a semi-conductor as its conductivity increases with temperature, and finally it becomes a fully ionized plasma at sufficiently high temperatures.

The ionization energy for the removal of the first electron from a gas atom varies from 4 to 5ev for alkali metals to 10 to 25ev for halogens and noble gases (1ev = 1 electron-volt = $1{\cdot}6 \times 10^{-12}$ ergs). The total energy E of the electron in a hydrogen atom can be written as the sum of potential and kinetic energies

$$E = -\frac{e^2}{r} + \frac{p^2}{2m}\,. \tag{5}$$

The electron tends to move towards the nucleus and so reduce the potential energy; but this effect is resisted by the kinetic energy, which rises as the space in which the electron moves is decreased. Heisenberg's principle shows that if the electron moves at a radius r from the nucleus its momentum cannot be smaller than $h/2\pi r$. Substituting this for p in eqn. (5) and minimising E with respect to r, we obtain

$$r=\frac{h^2}{4\pi^2 me^2}=0{\cdot}53\times 10^{-8}\text{ cm}, \tag{6}$$

$$\text{and } E=\frac{-2\pi^2 me^4}{h^2}=-13{\cdot}6\text{ ev}, \tag{7}$$

for the radius and energy of the lowest electronic state of a hydrogen atom; the ionization energy is $-E$.

As regards more complex atoms we recall that the distribution of electronic charge about a nucleus is best described by means of ' wave functions ' and that the principal quantum number n of such a function determines the radial distribution of the charge and hence mainly determines the energy of electrons in the ' shell ' of quantum states with that value of n. A striking feature of atomic structures is the formation of spherically-symmetrical filled shells of 8 electrons. Because of the spherically symmetrical way these electrons are disposed about the nucleus each electron in such a shell is about as near to the nucleus as any other and hence enjoys the electrostatic attraction of the nucleus with the least possible shielding or ' screening ' by its companions in that shell. The ionization energy is then large. Such structures occur in atoms of the inert gases. If the nuclear charge is reduced by one unit a ' hole ' corresponding to one missing electron appears in the otherwise spherical shell of the neutral atom. The ionization energy to remove one of the electrons in the shell remains high, but the atom now has the ability to attract one extra electron, so becoming a negative ion, to fill the hole and complete the spherical shell. Such atoms belong to the electronegative halogen family of elements. Conversely, with an alkali metal atom, which has one more nuclear charge than that of an inert gas, there is no room in the filled spherical shell for its extra electron and this is forced out into a quantum state lying further from the nucleus. So far as this electron is concerned, the effect of the extra nuclear charge is more than offset by the screening effect of the electrons in the filled shell which lie between it and the nucleus. It is thus easily ionized. The atom is then turned into a singly charged positive ion.

The affinity of electropositive and electronegative elements for one another, and the formation of ionic compounds and solids such as rock-salt, can thus be explained in terms of the transfer of electrons from one to the other and the electrostatic attraction of the ensuing positive and negative ions. The chemical valence of an atom is equal to the number of electrons which must be added or removed to bring it to the nearest filled-shell structure. When this is achieved the atom is ' chemically saturated '.

4. Insulators, semi-conductors, and metals

In covalently bonded substances each pair of neighbouring atoms shares two valence electrons and each atom bonds with enough neighbours to obtain the equivalent of a filled shell from these shared electrons. The atoms in diamond, silicon, and germanium, have four neighbours each and their chemically saturated bond structure can be represented by a diagram of the type shown in fig. 2, in which each pair of lines represents an electron-pair bond.

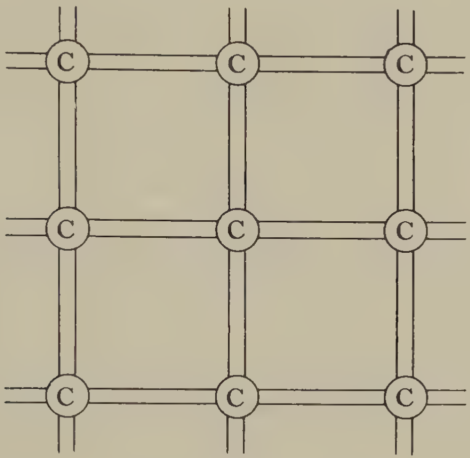


Fig. 2. Covalent bonding in diamond.

The electrons in these bonds cannot transport an electrical current without disarranging the bond structure. When it is disarranged, however, for example by photo-ionizing one of the valence electrons with a γ-ray, a pulse of current can be produced through the crystal; this effect is exploited in the ' diamond crystal counter ' of nuclear physics. The insulating properties of diamond must therefore be ascribed, not to immobility of the valence electrons, but to the resistance of the bond structure to disarrangement. Electrons can change places with one another but this does not produce a flow of current.

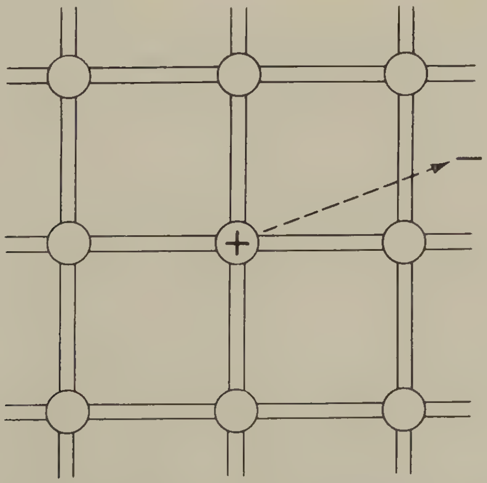


Fig. 3. Representation of a phosphorus atom in silicon as an unbonded electron and a positive ion.

As in gases, conduction can also occur through thermal ionization. Consider, for example, the behaviour of an impurity atom such as phosphorus built into the lattice of, say, silicon, as in fig. 3. Four of its valence electrons enter covalent bonds, but the fifth remains unbonded and is held electrostatically to the positive ion like the electron in a hydrogen atom. Eqns. 6 and 7 can be applied to describe this electron provided we allow for the effects of the crystalline field of balanced electrical forces which forms the ' space ' in which it moves. This is done in the calculations by replacing the actual mass of the electron by an ' effective mass ' m^*, a parameter that takes account of the fact that an electron moving through a periodic electric field does not have the same mobility as one moving through a smooth field, and by replacing the electrostatic interaction e^2/r with $e^2/\kappa r$. Here κ is the dielectric constant of the material, which allows for the fact that the covalently bonded atoms become polarized in the field of a free charge, their electrons being displaced slightly in the direction of the positive charge.

In silicon $m^* \simeq 0{\cdot}25m$ and $\kappa = 11{\cdot}9$. Eqns. 6 and 7 then give $r \simeq 25 \times 10^{-8}$ cm and $E \simeq {}^-0{\cdot}025$ ev. (In practice E is raised to nearly twice this value by subsidiary effects.) Due mainly to dielectric polarization the electron moves in a much larger orbit than in the hydrogen atom and is easily ionized. Ionization involves removing the electron far from its ' donor ' atom and so freeing it to take part in conduction. The electron is then said to have joined the ' conduction band ' of the material by the acquisition of the ionization energy, denoted in this case by E_d. Because E_d is small thermal ionization occurs appreciably even at room temperature. The material is not a metal however, because the conductivity disappears at low temperatures (cf. eqn. 4), but is an ' impurity semi-conductor '.

Intrinsic semi-conductivity, produced by the thermal ionization of electrons out of valence bonds, is also possible but requires a larger energy, E_g, made up of the excitation energy required to unbond a valence electron and of the electrostatic energy required to pull the electron away from the ' positive hole ' which it leaves behind in the valence bond structure. Some values of E_g (in ev) are as follows: diamond, 6·5; silicon, 1·1; germanium 0·7.

Such effects as these are usually represented by an energy band diagram, as in fig. 4, in which energy is measured vertically and distance horizontally. These bands are analogous to energy levels in free atoms; broadening into

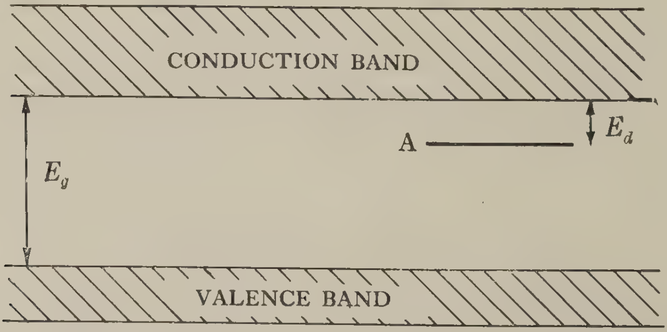


Fig. 4. Bands of electronic energy levels in a covalent solid. A localized level representing the bound state on a donor impurity atom is shown at A.

bands occurs because the closely spaced atoms interact with one another. An electron bound to a donor impurity atom is localized in a quantum state, belonging to that atom, which lies at a level E_d below the conduction band.

When large numbers of ionizations occur a new major effect sets in to accentuate further the degree of ionization—the material becomes too good a conductor to permit long-range electrical fields to exist inside it. The effect of this is rather like dielectric polarization but much more drastic; and the Coulomb interaction e^2/r is replaced, not by $e^2/\kappa r$, but by a ' screened charge ' interaction, given by

$$V(r) = -\frac{e^2}{r}\exp(-r/r_0), \tag{8}$$

where r_0 is of the order of magnitude of the spacing between the ionized electrons. Unlike e^2/r this is a short-range interaction which virtually disappears at distances larger than r_0. When r_0 is sufficiently small (e.g. of order of the atomic spacing) the screened potential is too weak to produce a bound state for an electron at all, and the ionization energy, which is the factor that prevents electrons becoming free at low temperatures, then disappears. As Mott (1958) has emphasized, this means that a sharp increase in the number of free electrons irrespective of temperature, leading to a metallic state of conduction, is to be expected when the screening radius becomes small.

There are several ways of increasing the number of ionizations so that screening can occur: (1) by heating to high temperatures (provided E_g is not so large that vaporization occurs first), (2) by increasing the concentration of suitable impurity atoms until the wave functions of the electrons in impurity bound states begin to overlap, and (3) by reducing the value of E_g.

It is the last of these that distinguishes metals from insulators. In a metallic solid or liquid, particularly of high coordination number, there are not enough valence electrons available to saturate the valence bonds. An electron ionized out of such a bond will, wherever it goes in the metal, always be able to form a similar bond in its new position since there are vacant bonding states everywhere. The unbonding energy which contributes so strongly to E_g in a chemically saturated material such as diamond is now absent. Every atom can behave as if it were a donor impurity, offering its easily removable valence electron(s) for ionization, and the ionization energy is eliminated by the intense screening which sets in. Conditions thus favour the complete disappearance of bound states for the valence electrons. The gap between the conduction and valence bands disappears and large numbers of free electrons exist at all temperatures. This is the metallic state.

5. EFFECT OF A PERIODIC CRYSTALLINE FIELD

The above theory of the difference between metals and insulators is due essentially to Mott (1958), and is closely related to a theory due to Pauling (1949). The crystallinity of the atomic structure plays little part in the argument. Since the distinction between metals and insulators persists in the liquid state, this is reasonable.

It has often been thought that crystallinity plays an essential part, however. There is a well-established theory (the ' zone ' theory) due to Bloch (1928) and Wilson (1931) which says that, because the electric field though a crystal is *periodic*, certain states of motion must be forbidden to electrons in crystals. If an external beam of electrons is allowed to impinge on a crystal, with a direction and momentum corresponding to one of these forbidden states, it is rejected by the crystal by reflection off a set of crystal planes. This is the basis of the phenomenon of electron diffraction. The forbidden states occur in certain energy bands so that energy diagrams similar to that of fig. 4 may be deduced purely from considerations of crystal symmetry and the periodic variation of potential through the lattice. A distinction between metals and insulators can then be drawn according to whether or not the allowed energy bands are partly filled by electrons.

The periodicity of the lattice field certainly does influence the behaviour of electrons in solids. Measurements of various physical properties have given evidence of the variation in the number of allowed quantum states and in the effective mass of an electron for different directions of motion through a crystal. Pippard (1957) has shown that these effects are strong even in such an ' ideal metal ' as copper. Evidence for crystallographic restrictions on the motion of free electrons is also provided by the behaviour of a poorly conducting metal such as bismuth; its conductivity increases sharply on melting towards a value more typical of metals generally. It is also known that many alloys of the type described as ' electron-compounds ' (e.g. CuZn, Au_5Zn_8, $AgCd_3$) form particularly at certain critical ratios of the numbers of free electrons to atoms (1·5, 1·61, 1·75) and that these ratios can be explained in terms of the effect of crystal structure on the energies of such electrons (Jones, 1937).

Important though these effects of the crystal structure are, one feels that they are subsidiary effects. Although insulators improve their conductivity on melting, they do not usually become metals in spite of the destruction of the long-range crystal structure on which the zone theory depends. The importance of ionization, rather than partly filled energy bands, as the criterion for distinguishing metals and insulators is shown by considering materials such as nickel oxide (Mott 1958). Nickel is a transition metal and in forming NiO by transferring two of its electrons to oxygen to form an ionic crystal, it must leave two places vacant in its outermost electronic shell (the 3d shell). In the solid the energy levels of this shell broaden out into a band which is only partly filled. This band would therefore be expected to provide conductivity on the zone theory whereas in fact nickel oxide is an insulator.

6. Cohesion in metals

The picture we have then of a simple (non-transition) metal is one of essentially spherically symmetrical positive ions floating in a sea of mobile free electrons. The free electrons are provided by the easily ionized valence electrons of the atoms, those electrons belonging to the filled shells requiring too much excitation energy to be lifted out of their shells into the conduction band. Since there is no saturation of the bonding states for the valence electrons the $(8-N)$ rule no longer restricts the number or type of neighbours which an atom may have. Close-packed structures and free intermingling of different metallic atoms in alloys are thus expected. The bond that an atom makes with any single neighbour is weak but this is compensated by the stacking of the atoms in close-packed formations so that each has many neighbours and the overall cohesion is strong. Thus metals compare well with ionic and covalent solids as regards melting points, boiling points, elastic constants, and other cohesive properties.

This tendency towards close-packing leads in pure metals to the simple crystal structures of fig. 1. In certain alloys such as *Laves phases*, e.g. $MgCu_2$, $K\,Na_2$, advantage is taken of the difference in the sizes of the participating atoms to form more complex crystal structures of even closer packing in which the coordination number exceeds the maximum (12) possible for equal-sized spheres. The ability of metals to dissolve very small atoms (e.g. H, N, C) interstitially, i.e. in the spaces between the metal ions, which is of great technological importance in the case of carbon steels, is another example of this effect of the unsaturated metallic bond.

The flexibility of the metallic bond influences the surface and elastic properties of metals. If a metal is cut into two pieces along some surface the electrons previously responsible for the bonds through that surface can transfer themselves to bonds between atoms in each of the new surfaces so created. The surface energy of a metal is thus smaller than would be expected purely from the number of bonds cut in making a new surface. A similar argument holds for elastic constants. There are two basically different ways of deforming a solid elastically; by change of volume at constant shape (dilatation) and by change of shape at constant volume (shear). Resistance to the first is measured by the bulk modulus of elasticity, that to the second by the rigidity modulus. The flexibility of the metallic bond allows atoms in metals to slide over each other fairly easily and the ratio of rigidity modulus to bulk modulus is lower for metals than for most other solids. A convenient measure of this is provided by Poisson's ratio, the ratio of lateral to longitudinal strain when a bar is stretched in simple tension. Lateral contraction decreases the dilatation but increases the shear. Poisson's ratio for a liquid with no resistance to shear is 0·5; for the elastic deformation of a metal it is usually about 0·33 whereas for diamond glass, and ionic solids, it falls generally in the range 0·2 to 0·3.

To discuss the cohesion of metals quantitatively it is better to think of it in physical terms, as the coherence of positively charged ions to a negatively charged fluid, rather than chemically, as the direct bonding of atoms to one another. When free atoms condense together to form a metal their valence electrons become free to explore the whole material. The localization of such an electron to a particular atom disappears, and with it disappears the kinetic energy due to this localization (cf. eqn. 5). The potential energy on the other hand is not much altered since the electron is never far from a positive ion. In a free atom of an alkali metal the potential energy (negative) is about twice the magnitude of the kinetic energy (positive) so that, when the atoms condense together, it is possible for an electron practically to double its binding energy. The actual increase in sodium due to this effect is 3·13 ev.

Most of the valence electrons do not do as well as this, however. There are two opposing effects. First, because these electrons all share a common space, the Pauli principle restricts them to two electrons (with opposite spins) in a quantum state of this space. The result is as described in Sommerfeld's theory (section 2). The electrons occupy all states in a band of levels up to a maximum, called the 'Fermi level', which is 3·12 ev for sodium. For an electron at the top of this band, therefore, the Fermi energy almost exactly cancels the gain in binding energy. However, the average Fermi energy of the electrons is only 0·6 of this maximum value, i.e. 1·97 ev. There is thus a net binding energy of $3{\cdot}13 - 1{\cdot}97 = 1{\cdot}16$ ev, which is strikingly close to the measured vaporization energy of sodium (1·13 ev per atom).

The Pauli principle keeps the electrons out of one another's quantum states but does not otherwise prevent them from occupying the same point in space. Since electrons interact with one another electrostatically with a long-range potential e^2/r it would seem that their energy of interaction might make a strong contribution, additional to the Fermi energy, opposing the cohesion. In fact, early attempts to develop a free electron theory of cohesion ran into great difficulties at this point. The effect, however, turns out to be almost negligible. The long-range part of the interaction can be shown to lead to a coherent oscillation of the whole electron gas, called a ' plasma oscillation ' (Bohm and Pines, 1951-53). This is a very interesting phenomenon, but it hardly alters the cohesive energy. When the part of the interaction represented by these plasma oscillations has been accounted for there remains only a short-range interaction very similar to that of eqn. 8. What happens, in effect, is that the electrons correlate their positions with one another so that each carries about with it a ' positive hole ' in the electron distribution in which another electron is unlikely to be found. If it were possible to measure it, the average density of electronic charge along a line through such an electron would look rather as in fig. 5. The central spike represents the electron and the area beneath it is balanced by the reduced area beneath the depressed region of the electron distribution nearby. As regards cohesion the result for a monovalent metal is that when a given electron happens to lie on a given ion there is hardly ever another free electron on that ion. There are, of course, free electrons on neighbouring ions but their electrostatic interaction with the electron in question is almost exactly cancelled by the interaction of these other ions with it. The only important interaction of the electron is that with the ion on which it happens to lie, as in a free atom.

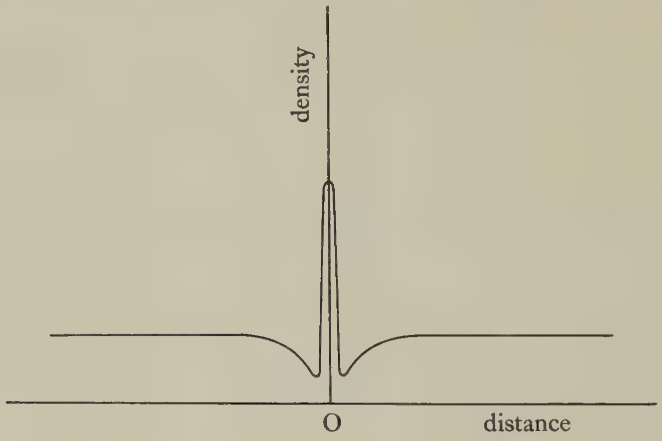


Fig. 5. Expected variation in density of electronic charge along a line through a free electron.

7. CLOSE-PACKED STRUCTURES

It is a long step from free electrons to the engineering properties of metals, but the shortest route lies through the metals copper, silver, and gold. In these the free electron gas pulls the ions together until the filled electron shells of neighbouring ions begin to overlap and exert a strong resistance to further interpenetration. This ion-ion repulsion, which is mainly responsible for the observed lattice constants of these metals, is a short-range ' central ' force, i.e. it acts between nearest neighbours along the line between their centres. The free electrons, by contrast, are sensitive essentially to the *volume* allotted to each atom rather than to the distance between neighbours.

The need to keep the volume small and the ionic spacing large is best satisfied when the atoms are packed in a structure of maximum coordination number. For a plane sheet of equal spheres there is only one such structure, as represented by the circles centred at positions A in fig. 6. The face-centred cubic (F.C.C.) crystal structure can be built up by laying several such planes on top of one another so that the atoms of one rest in the hollows of another. However, there are always two alternative sets of such positions, e.g. those marked B and C in fig. 6. Thus there are many possible ways of stacking the planes in a fully close-packed manner. The sequence . . . ABCABC . . . represents an F.C.C. structure, the reverse one . . . CBACBA . . . its ' twin '. Then there is the close-packed hexagonal (C.P.Hex.) sequence . . . ABABAB . . . , and irregular sequences such as . . . ABACBABCBAC . . . , which all satisfy the same criterion of close packing.

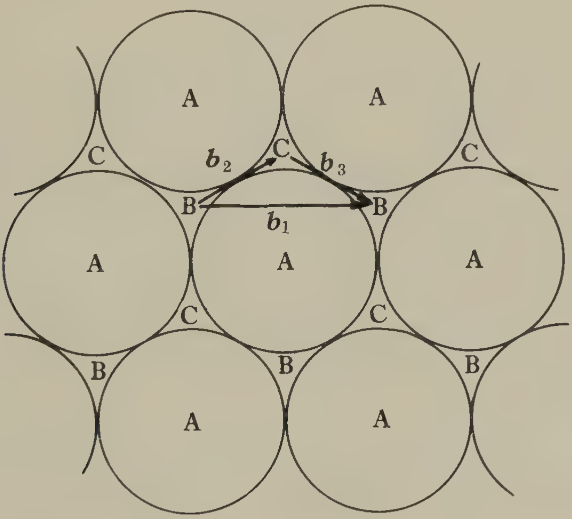


Fig. 6. A close-packed plane.

Since copper, silver, and gold, do form F.C.C. crystals there must exist in them something sensitive to the relative positions of more distant neighbours, e.g. electrostatic interactions of ions and electrons in different lattice cells. From our previous arguments we would expect these to be small effects, however, and this is confirmed by the ease with which these metals can be made to form ' annealing twins ' and ' stacking faults ' by various working and annealing treatments (Barrett, 1952). An annealing twin consists of a reversal of the stacking sequence, as shown schematically in fig. 7, whereas a stacking fault may be regarded either as two such reversals, one immediately following the other,

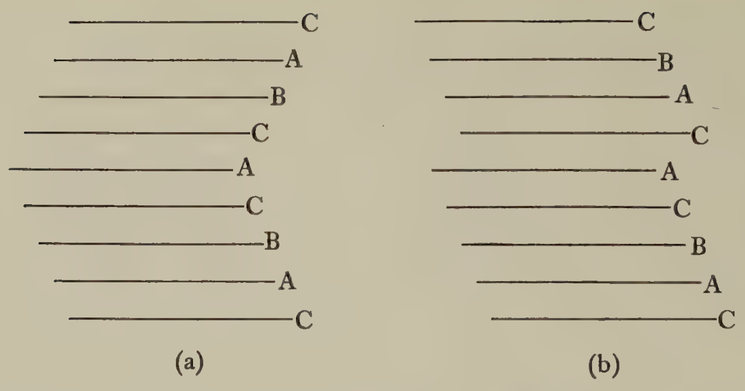


Fig. 7. A twin (*a*) and a stacking fault (*b*) in an F.C.C. sequence of close-packed planes.

or as the result of slipping one half of the crystal relative to the other along a vector such as $\boldsymbol{b}_2$ in fig. 6.

8. Strength and plasticity of metals

Metals are strong because they are soft. It is the glass bowl that breaks, not the silver one, when dropped on the floor, even though the silver can be easily scratched by the broken fragments. To resolve this paradox and understand mechanical properties it is necessary to distinguish between the inherent strength of matter and the overall strength of large pieces. A convenient measure for comparing strengths of different materials is the elastic *strain* at failure. When multiplied by the appropriate modulus of elasticity this gives the failure stress. Silicon is brittle at temperatures below 600° C, but at this temperature its elastic failure strain is almost 1 per cent. Few metals and alloys withstand *elastic* strains as large as this before they break. The reason why brittle materials appear weak in practice is that ordinary samples usually contain small notches and other stress concentrators so that, locally, elastic strains of order 1 per cent. are reached even while the overall applied load is still quite small. A soft, ductile metal, by contrast, usually begins to yield plastically at an elastic strain of order 0·1 per cent., and this smooths away the stress concentrations at notches before they become dangerously large.

Plastic deformation occurs by the slipping of crystal planes over each other, often by hundreds of atomic spacings, as shown rather simply in fig. 8. The slip takes place along a certain crystallographic ‘ slip direction ’ and usually on a

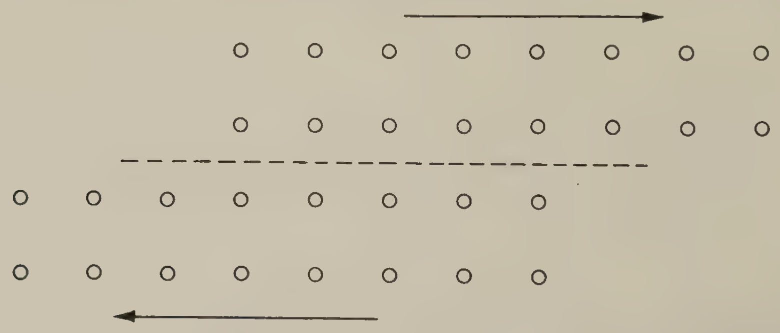


Fig. 8. A simple slip process.

crystallographic ' slip plane '. After sliding, the atoms settle into positions alongside their new neighbours and the original crystal structure is usually fully reconstituted in the slip plane. In F.C.C. metals slip usually follows the close-packed directions on the close-packed planes. The vector $\boldsymbol{b}_1$ in fig. 6 represents in fact the smallest ' slip vector ' that can reconstitute the original crystal structure. Close-packed directions and planes are preferred because, in a sense, they enable the sliding atoms to ' ride smoothly '.

The free electron bond favours the slip process for two reasons. First, because it does not oppose the sliding of one atom over another, so long as they do not pull apart. There are no interatomic bonds to be broken since the atoms are bonded to the free electron gas, not to each other. The close-packing requirement in metals such as copper, silver, and gold, is best satisfied during slip if the atoms move in a series of zigzag steps, along the vectors $\boldsymbol{b}_2$ and $\boldsymbol{b}_3$ in fig. 6 rather than along $\boldsymbol{b}_1$. Slip in this case is closely related to the faulting process shown in fig. 7 (b).

The second and more important effect of free electrons is in providing simple close-packed crystal structures which have smooth planes and directions for slip. It is this that makes the material soft and ductile. When the crystal structure is complex, as in Laves phases and related substances such as $CuAl_2$ and ' sigma ' phases in certain alloy steels, the material is hard and brittle despite the fact that it is made from good metals and bonded by free electrons.

9. DISLOCATIONS

To follow the story further we must examine the detailed atomic movements that occur during slip. Crystals are elastically flexible objects and it takes time for an elastic disturbance to spread from one side of a crystal to another. It is thus virtually impossible for all the atoms on a slip plane to slide simultaneously. Rather, slip begins in one small area of the plane and then spreads radially outwards from it, over the rest of the plane, like a wave spreading across the surface of a pond. As shown in fig. 9, while this process is taking place the slip plane is divided into a slipped area, which is bounded by the slip front or ' dislocation line ' as it is called, and an unslipped area outside this line.

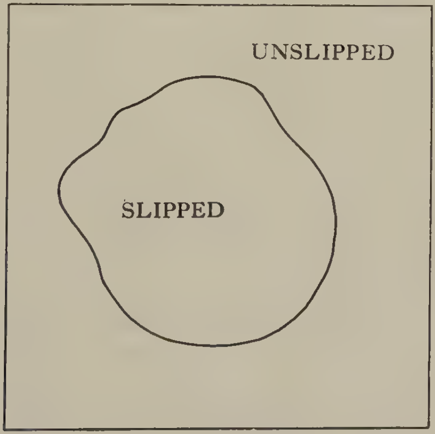


Fig. 9. Plan view of a plane in process of slip, showing the slipped and unslipped areas, and the dislocation line which forms the boundary between them.

Recent experiments have amply confirmed theoretical predictions about the occurrence of slip by the generation and propagation of dislocation lines. Slip occurs mostly by the propagation of ' unit ' dislocations each of which produces one quantum of slip, e.g. equal to the vector $\boldsymbol{b}_1$ (cf. fig. 6) on a close-packed plane. It has also been proved that in most crystals, and certainly in engineering metals and alloys, natural imperfections of the structure act as good sources of dislocations and that the process of slip itself increases the dislocation content of the material. There is thus never any shortage of dislocations in such materials and the yield stress is essentially the stress required to make these dislocations glide across their slip planes despite various obstacles that stand in their way (see, for example, Read 1953, Cottrell 1953).

The first obstacle to consider is the periodic field of atomic forces in the crystal. Fig. 10 shows the atomic structure in a section through one type of simple dislocation. Such structures have now actually been resolved in certain materials with the electron microscope (Pashley, Bassett, and Menter, 1958). Atom A is in the half-slipped position, sitting vertically above B at the centre of the dislocation. We may suppose the atoms to the left of A to have slipped already, towards the left. To move the dislocation by one atomic spacing towards the right, atom C has to be pulled up vertically above D; atom A then slides down to the same position relative to B that E occupies at present relative to F, and correspondingly smaller adjustments are made in the positions of more distant atoms.

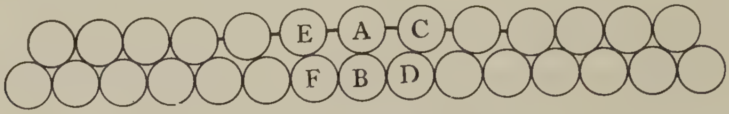


Fig. 10. A simple dislocation.

In fig. 10 atoms C and E are symmetrically positioned about the centre of the dislocation. Atom E has almost completed its slip movement but, since it can fully complete it only if the dislocation glides away to the right, it exerts a force on the dislocation pushing it forward. But by symmetry, atom C exerts an equal and opposite force pushing the dislocation backward. The same holds for all other symmetrical pairs of atoms about the dislocation. In this position of the dislocation, therefore, the periodic crystal forces offer no resistance to its motion.

This is not true however for all positions of the dislocation. In transferring its centre from A to C the dislocation passes through unsymmetrical configurations in which the atomic forces no longer cancel in pairs. There is then a resistance to the movement of the dislocation, and it turns out that this is large when the dislocation is *narrow*, i.e. when the width of the belt of badly disarranged atoms in the slip plane, e.g. from E to C in fig. 10, is small. Narrow dislocations are expected when the atoms are sensitive to their relative positions, and the hardness and brittleness of substances such as diamond and silicon is largely due to this effect.

The free electron bond favours wide dislocations and a small lattice resistance to slip, since it is insensitive to the relative positions of the atoms. Ideal conditions for wide dislocations are found on close-packed planes in metals. Not only is the field of periodic atomic forces particularly smooth on such planes; the atoms are also provided with an excellent half-way position. In sliding according to the vector $\boldsymbol{b}_1$ in fig. 6, for example, the atoms may move first to C by the vector $\boldsymbol{b}_2$, and then complete their unit of slip, using the vector $\boldsymbol{b}_3$. This gives the dislocation the opportunity to spread out into a wide ribbon, rather than a narrow line, in the slip plane. The leading edge of this ribbon then produces a slip $\boldsymbol{b}_2$ into the faulted position, the centre of the ribbon consists of a stacking fault, and the trailing edge produces the remaining slip $\boldsymbol{b}_3$. In copper, silver, and gold, the width of such dislocations is about 10 atomic spacings. In α-brass (e.g. 70 per cent. Cu, 30 per cent. Zn), F.C.C. stainless steel (e.g. 18 per cent. Cr, 8 per cent. Ni, in Fe), and several related alloys, even wider dislocations are obtained. In some cases the leading and trailing parts of the dislocation are so easily separable that large areas of stacking fault can be produced across the slip plane.

Such materials are intrinsically soft. Any hardness they possess is due to alloy and impurity atoms, foreign particles and precipitated phases, grain boundaries and lattice strains, which all serve as obstacles to the motion of dislocations. The more the material is purified and the structure annealed, the softer it becomes, and this softness persists even at the very lowest temperatures and under impact conditions of loading.

Such materials also work-harden strongly; i.e. the lattice strains produced as a result of plastic working cause the yield stress for further deformation to increase strongly. Work hardening, which is a valuable engineering property, occurs essentially when dislocations moving on intersecting slip planes meet and obstruct one another's motion; dense traffic jams of dislocations then build up at such places of intersection. This effect is particularly pronounced when the individual dislocations split apart to form wide stacking faults. Such a fault provides an almost impenetrable plane barrier to dislocations trying to glide through it on other planes. Very high hardnesses can be developed in this way without the material becoming excessively brittle.

The other main method of hardening such metals is through controlled precipitation of fine particles in the crystals by careful alloying and heat-treatment methods. Sometimes these particles are formed right on the dislocation lines and anchor them in position. Often they are dispersed throughout the entire crystal and provide a series of obstacles which the dislocation must overcome in its passage through the slip plane. One important class of creep-resistant alloys for jet engines consists in fact of F.C.C. stainless steel hardened by fine precipitates of such things as, for example, niobium carbide.

The wideness of the dislocations in such materials is also important for their high-temperature strength. At temperatures high enough for solid-state diffusion to occur many more opportunities exist for dislocations to by-pass obstacles than at, say, room temperature. Most materials become soft under these conditions. This by-passing occurs mainly by movements of dislocations in directions *out* of their slip planes and such processes can occur only with difficulty when the dislocations exist as broad ribbons in their slip planes. It is hardly possible to move such a ribbon, at *any* temperature, in any direction other than in its own plane, and if the movement in this plane is obstructed by fine precipitates that persist at high temperatures, the material will remain strong even when hot. This is the basis of a large class of modern creep-resistant alloys. The starting material is an F.C.C. metal or alloy of high melting point that forms wide dislocations. Further alloy additions are then made to it, some which may widen the dislocations further, e.g. cobalt, others which may form stable high-temperature precipitates, e.g. alloy carbides.

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The Clue to a Stellar Interior from Apse Motion of a Binary Orbit

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Summary

A group of researches has recently been linking the mutual dynamical interaction of a close pair of stars with the nuclear physics of the internal stellar structure. The rate at which the apse line of their orbit alters its orientation depends on the tidal and centrifugal distortion of such a closely associated pair ; but for given dimensions of the two stars and their orbits this apse rotation is slower the more concentrated the material towards the centre of each star. This in turn depends on the evolutionary stage reached in depletion of their hydrogen as source of energy. The topic is here traced from the earliest papers by Russell in 1928 to the recent ones by Princeton astronomers in 1958. Critique of the potentialities of such work is finally suggested.

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1. A LINK BETWEEN GRAVITATIONAL ASTRONOMY AND NUCLEAR PHYSICS

Of two epochs of research, nearly three centuries apart along the list of debts owed by physics to astronomy, the earlier is marked by elucidation of gravitational law from orbits and the later by elucidation of thermonuclear reaction from the need to envisage sources of energy for a radiating star. Gravitational orbits might be thought nowadays to offer mathematical refinement rather than physical novelty, compared with the rapidly developing mystery of how a star evolves while converting its hydrogen into helium ; indeed the modern physicist sometimes thinks astrophysics interesting and astronomy not, and research is obstructed by their separation. So the present paper attempts some critical survey of a topic in which the older dynamical joins the modern nuclear problem, and in which astrophysics cannot be divorced from Kepler's laws. The situation is this : when the orbit of a closely associated pair of stars or binary can show a progressive drift in orientation of its line of apsides or major axis, the ratio of periods P/P' is a function of the distribution of material in the stellar interior. P' measures the cycle of complete rotation of the apse, P the far briefer revolution of each star about the common mass-centre. Hence, in principle, if P/P' becomes observable by modern photometric technique, there emerges a clue to evolution in terms of the degree of exhaustion of hydrogen as the fuel in a stellar core.

The purely mechanical problem involved is that an elliptic orbit can only be an exactly repetitive path about its focus if the latter is strictly a point source of attraction, and only a perfectly spherical sun or star or planet which also has a monopoly of field completely free from external perturbation can act as such a point source. If this requirement is not satisfied, the orbit is not perfectly closed and so the apse line gradually swings round (fig. 1) ; in completing its cycle of orientations in the period P', the point of nearest approach of satellite body to attracting body occupies in turn all positions around a circumference of that radius. Well-known instances can be listed from astronomy, relativity, and electron orbits. The moon with monthly orbital period rotates its apse in

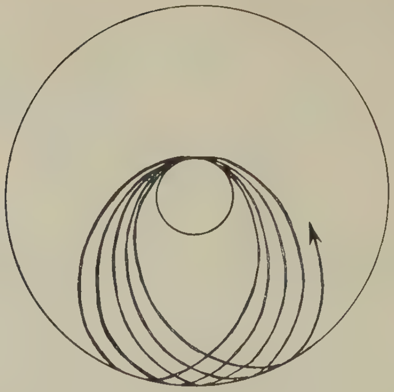

Fig. 1. Apse motion of elliptic orbit.

about 9 years, but the earth is so nearly spherical that the main cause here is perturbation by the sun. On the other hand, the oblateness of Jupiter, acting on a satellite where P is 12 hours, causes rotation of apse of the orbit in about 100 days. The orbital speed of Mercury is so great when nearest to the sun that the variation of mass with velocity discussed in Special Relativity enforces a similar apse migration ; the fact that this observed drift of perihelion exceeds that expected is taken as support of General Relativity where the slight divergence from Newtonian simplicity becomes threefold. The first relativistic variation of mass with velocity involves an even larger disturbance of inverse square simplicity, and therefore an apse rotation, in the traditional orbital model of electron and proton in the hydrogen atom, and has been associated with the fine structure of the optical spectrum. And apse rotation of the first Sputniks impressed this divergence from a repetitive stationary orbit even upon the non-scientific public.

The first astronomer to consider the physical implications of this apse rotation in the case of a closely associated stellar pair or binary was H. N. Russell of Princeton in 1928. The source of disturbance is here not a relativistic divergence from inverse square simplicity, nor attraction of an external body since we are omitting triple and multiple star associations, but the mutual tidal attraction between the two gaseous stars distorting the ellipsoidal shape which even a single star exhibits in the polar flattening due to spin. No star, contracted by its own gravity and yet distended by internal pressure and energy production, can have uniform density, and the apse rotation is slower, or P/P' smaller, the more concentrated the material towards the stellar centre ; since at extreme central concentration the attraction for the companion star approximates more nearly to the case of mass-point and sphere. Hence arises a unique clue to the inaccessible internal structure of a star, from observing the value of P/P' and comparing it with theoretical rates of apse rotation for hypothetical degrees of concentration inside a binary liable to some given dynamical distortion from spherical shape. The next step into evolutionary stellar physics depends on the fact that such gradation of density within a star will be a function of how and where hydrogen has been transformed to helium and energy released ; this latter problem developed since Russell's purely dynamical work, but has been notably handled in the last ten years by associates of Schwarzschild also at Princeton.

2. APSE ROTATION AS A PHOTOMETRIC FACT

We state later what knowledge of the binary is required in addition to the periods P and P', but these two main facts are purely photometric. Binary stars capable of yielding relevant data are restricted to those whose orbital plane lies as a line across our line of sight ; this orientation is guaranteed by finding that the two stars mutually eclipse one another in turn, thereby causing two dips, both repetitive, referred to as primary and secondary, in the time-graph of the light received. The time for complete repetition of the two dips is the quantity we called P.

Discovery of the much longer P', the period of completion of the cycle of orientations of the apse, is more difficult and hard to measure. We can only use binaries in which P is short, unless P' is to be many centuries long. Fortunately, values of P of few days or a week denote close proximity of the two stars, favouring maximum mutual distortion and not too lengthy P'. It must be remembered that the pair constitute only a single point source of light photographically : any binary system whose stars could be separately seen would have P itself in perhaps many years and P' impracticably long.

For P suitably brief, P' reveals itself as a slowly periodic displacement of the secondary eclipse dip in the light curve between previous and succeeding primary eclipses. The sequence can be illustrated by the convenient device of picturing the orbit of the lesser star around an artificially fixed greater star, although actually they both revolve about the common mass-centre (fig. 2).

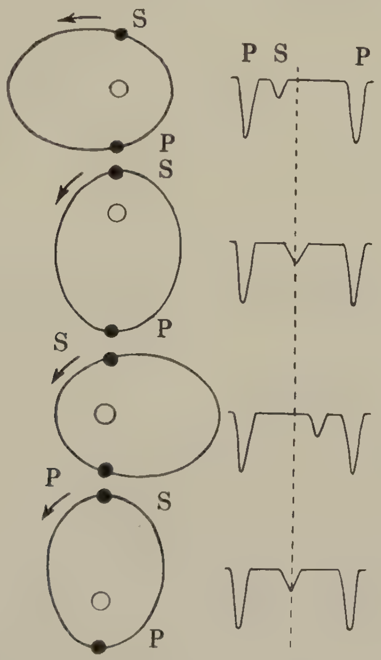

Fig. 2. Detection of apse rotation by periodic displacement of secondary eclipse (S) between primary eclipses (P).

Three effects arise from the dynamical fact that at opposite ends of the line of apsides, namely, at periastron (nearest to focus) and apastron (furthest from focus), the velocity in the orbit has maximum and minimum values respectively ; the three effects repeat with period P' each time the orientation of the orbit completes its cycle of apse rotation across our line of sight. (i) The time elapsing between eclipses of primary by secondary star and of secondary by primary does not remain constant, the secondary eclipse being in turn nearer to the preceding primary and then to the succeeding primary, as shown in our first and third photometric traces. Accurate continuous photometry, by a recording photomultiplier unit, can reveal two associated smaller phenomena, (ii) duration of the dip signalling an eclipse will be briefer at the faster transit, (iii) with large eccentricity the descending and ascending sides of the dip may not be quite identical through the rapid change in velocity.

If the star is also a ‘ spectroscopic binary ’ and the velocities of the pair can be separated by the Doppler displacement of their spectral lines, asymmetry in the contours showing periodically fluctuating speeds will also occur.

3. EXPRESSION OF THE OBSERVATIONS IN TERMS OF AN APSE CONSTANT AND A POLYTROPE INDEX

We now outline briefly some steps in the argument relating any observed P/P' to properties in the structure of a star, with illustrative dynamical detail from the simple pioneer treatment by Russell, reporting even more briefly later improvements suggested up to the present day.

If P in the first consideration is regarded as the orbital period of a small satellite about an attracting primary whose ellipticity is e or $(d-d')/d$ relating maximum and minimum diameters, a is mean radius of orbit and r mean radius of attracting body, let ϕ be the ratio of centrifugal force to gravity at the equator of the primary. Then the point of nearest approach of secondary to primary (perigee for moon and earth, perihelion for planet and sun, periastron for secondary star in a binary) will migrate completely round in P', and the ratio of periods is given by

$$P/P' = (r^2/a^2)(e - \tfrac{1}{2}\phi)$$

A first connection with the internal structure of the primary is at once implied, since for a homogeneous body $e = \frac{5}{4}\phi$, whereas for concentration of the entire mass at the centre $e = \frac{1}{2}\phi$ so that the bracket becomes zero and the apsidal period infinite.

In the primitive stage of stellar physics when mean density $\bar{\rho}$ is considered ; that quantity is $M/\frac{4}{3}\pi r^3$ where M is total mass. Since centrifugal acceleration is $4\pi^2 r/t^2$ where t is period of spin of the primary, and gravitational acceleration at the surface is GM/r^2 where G is the Newtonian constant, ϕ becomes $3\pi/G\bar{\rho}t^2$.

But mean density is not enough, and distribution throughout the star must be investigated if any attempt is made to write a stellar life-history. Towards that aim a more useful quantity than ϕ was introduced by Russell and has since been accepted as the ‘ apsidal motion constant k’, where $(e - \frac{1}{2}\phi)$ is replaced by $k\phi$. The relation of k to internal and evolutionary parameters in stellar physics has been the modern problem of binary apse motion inherited from Russell.

Russell adapted the above simple account of a *small* secondary body in orbit around a massive centrifugally distorted primary, by rewriting the equation for P/P' to express k and to allow for mutual attraction of two oblate spheroids ; for in stellar binary pairs the one mass is no longer negligible compared with the other. Using radii r_1, r_2, for the two stars, spin parameters as before but ϕ_1, ϕ_2, and the common separation a, we have as dimensionally equivalent to the simpler equation,

$$P/P' = \frac{1}{a^2}(k_1 r_1^2 \phi_1 + k_2 r_2^2 \phi_2)$$

If the stars have the usual property that periods of spin and of revolution coincide and their masses are m_1, m_2,

$$\phi_1 = \frac{m_1 + m_2}{m_1} \frac{r_1^3}{a^3}$$

and hence

$$P/P' = \frac{m_1 + m_2}{a^5} \left\{ k_1 \frac{r_1^5}{m_1} + k_2 \frac{r_2^5}{m_2} \right\}$$

This would be a sufficient account if the distortion of the stars were due solely to centrifugal force. Allowing also for an upward tidal force which is $2m_2/(m_1+m_2)$ times the centrifugal at the nearest point of one star to the other, and a downward force of half the value at 90 degrees from that point, the tidal distortion of diameter can be added to that due to centrifugal force, so that

$$P/P' = k_1 \frac{r_1^5}{a^5}\left(1 + 7\frac{m_2}{m_1}\right) + k_2 \frac{r_2^5}{a^5}\left(1 + 7\frac{m_1}{m_2}\right)$$

This we may compare with the very similar equation used in the most recent publication (Schwarzschild 1958)

$$P/P' = 15\left(k_1 \frac{r_1^5}{a^5} \frac{m_2}{m_1} + k_2 \frac{r_2^5}{a^5} \frac{m_1}{m_2}\right) \left\{ \frac{1 + \frac{3}{2}\epsilon^2 + \frac{1}{8}\epsilon^4}{(1 - \epsilon^2)^5} \right\}.$$

Here the final bracket introduces explicitly the eccentricity of the *orbit*, ϵ, which had appeared in the most elaborated pre-war treatment (Sterne 1939).

At the time of these pioneer studies by Russell, evaluation of the apsidal constant k from observation of P/P' and knowledge of stellar masses and dimensions could only rely on one instance, the star Y Cygni, and for this Russell estimated that the 'observational' k would be 0·076. For a star of uniform internal density the value would be 0·75 and at the other extreme would become zero if the mass were completely concentrated at the centre, and Russell's suggestions as to the possibilities of structural inferences were formulated as ratio of central to mean density ρ_c/ρ_m. Such a ratio is expressible by means of the 'polytrope index', for an important range of stellar models, and Cowling (1938) made the problem less artificial by treating these ellipsoidal stars not as static bodies but as capable of oscillation. He employed a formula of Chandrasekhar relating the apsidal constant k to the polytrope index n.

This index, developed by Emden and Eddington, regulates the gradation of thermodynamic properties from surface to centre of a star; the gradation is imposed by inward gravitational shrinkage against outward pressure from central energy sources. For example, in the unattainable limit of uniform density throughout, the ratio ρ_c/ρ_m would fall to unity and n tend to zero. At $n=1$ the ratio would be 3·29, at $n=2$ it would be 11·4, at 3 it becomes 54·4, at 4 it is 622 and at 5 it would be infinite. Russell obtained for his first treatment an improbably low central concentration. Cowling, adding the only other star then sufficiently known to test, GL Carinae studied by Shapley and Swope, obtained 42 for the density ratio, nearer to the central concentrations suggested on other grounds by Eddington.

The treatment of the whole subject, before likely nuclear reactions had been computed for a stellar centre, ends with the detailed elaboration by Sterne at Harvard (1939). He inserted the term for eccentricity of orbit which we quoted from Schwarzschild in 1958, and he distinguished between apsidal constants k_1, k_2 - - - k_j to allow for deformation of the stars to successive orders of approximation. His k_2 is the one corresponding to the simpler treatments of Russell and Cowling. Again employing the tables of Chandrasekhar, his values of k_2 for polytrope indices 1, $1\frac{1}{2}$, 2, 3, 4, are 0·2599, 0·1446, 0·0741, 0·0144, 0·00134, the value for $n=3$ agreeing with that obtained by Russell for appropriate density ratio from simpler argument.

To extract k or its associated polytrope index as an ‘ observable ’ quantity, it is clear from the equations we have quoted that other facts about a star beside P/P' are needed, and methods for determination of mass, size, and orbital separation are described in the astronomical textbooks for other purposes and will not be discussed here. The limitation so far observed by all workers is to suppose k identical for the two stars : this excludes many interesting types of binary, for which, however, P/P' itself is lacking.

In a later paper Sterne collected such data for five binaries. As regards expectation from theory, he computed for polytrope indices 1, 1·5, 2, 3, 4, the reciprocal of the previous fractional ratio, giving P'/P of 299, 541, 1057, 5470, 59 100 ; these figures show the variation of apse rotation with n and k alone, a one-parameter family. By allowing for particular radii, eccentricities, and masses, he then obtained from observed P'/P the following estimates of polytrope index :

Star:	*P'/P from observations:*	*n calculated:*
Y Cygni	5700	3·02
GL Carinae	3800	3·01
HV 7498	59 600	3·46
YY Sagittarii	39 200	2·83
V 523 *Sagittarii*	33 000	3·80

Comparison with Russell and Cowling indicates in general a somewhat steeper concentration of densities towards the centre of a star in the conclusions of Sterne. It had long been known in the studies of stellar constitution begun by Eddington that n is unlikely to diverge radically from 3, for stable stars of the simplest internal structure ; so the first question raised by the study of apse motion in the 1940's is whether any significance is to be read into small divergences. We mention next, therefore, some attempts at assessing real and fictitious accuracy.

4. METHODS FOR PRECISION IN LATER WORK

(i) When polytrope indices become quoted to three significant figures, and values of k range steeply for small changes in n, the relations between these two constants require to be freed from ambiguity ; a paper by Brooker and Olle (1955) has made precision possible in tabulating k_j or k for the j^{th} order of correction for all significant values of n.

(ii) Meanwhile Kopal (1953), in whose institution Brooker and Olle worked, had assessed critically the rate at which k changes around extreme values of the parameters expressing gradients in a star. This fixes limits of applicability for the equations of perturbation of gravitational orbits which had been inherited from the nineteenth century.

(iii) On the observational side, recalling that P' may cover many centuries, de Kort (1955) developed geometrical methods for extracting reliably a value for P' even when only a small fraction of the cycle of apsidal migration is observable.

(iv) Also on the observational side, Keller (1948) and Keller and Limber (1951) stabilized a list of eight stars which by critical assessment of their data seem worth comparing with theory, and themselves evaluated the facts for YY Sagittarii to an accuracy which sets an example for future work. They allow for maximum differences in interpretation of their own light curves and the measurements of Zinner, Kordylewski, Shapley and Swope, and they include a critique of the degree of sensitivity of n to uncertainties in the orbital elements. For example they compare a possible P' of 312 years with a maximum allowable extension to 360 years, solving the equations by three methods for each. This yields values of n for the 312 years of 3·52, 3·51, 3·55, and for the 360 years n becomes 3·57, 3·56, 3·59. It is notable that the mean 3·55 shows the greatest discrepancy in the whole history of the subject, when compared with the estimate by Sterne a dozen years before of 2·83 for the same star using earlier measurements.

A few further stars are mentioned in the Moscow report of the International Astronomical Union (1958), and notably Van Wijk has studied GL Carinae by more modern methods : the earliest data for this star by Shapley and Swope at Harvard gave one of the first stimuli to the subject.

(v) These advances in rigour of observation and inference in so difficult a chain of argument make more hopeful today's attempts at correlating apse motion of binaries with their evolution. A step required has been the abandonment of the simplicity which had characterized a whole star by a single polytrope index n ; the first adaptation of apse motion to the modern view that a stellar interior is probably composite or of multi-phase structure is associated with papers by Motz (1941, 1950, 1952) and by Keller. Motz also developed the dynamics which writes the apsidal constant k in terms of radius of gyration of the star. To see whether these refinements of the original Russell-Cowling-Sterne treatment permit intrusion of stellar age and evolution into the story requires next some brief account of the state of stellar physics at this time.

5. Nuclear energy sources and the core of a composite star

Ever since 1939, when Bethe and Weiszäcker first evaluated specific nuclear transformations and their temperature dependences, it has been recognized that at the temperatures estimated for polytrope centres by Eddington or Chandrasekhar the thermonuclear production of helium from hydrogen is a very probable source of the energy output seen in the luminosity of many common stars. In particular, the reaction known as *CN*, since isotopes of carbon and nitrogen contribute to the loss of hydrogen and gain of helium, (fig. 3) will be brisk at temperatures exceeding fifteen million degrees, and the simpler production of helium through proton and proton and deuteron encounters may replace the *CN* at lower temperatures. Both reactions fail before temperature is reduced to a million degrees, and although reactions with lithium, beryllium and boron may be important during brief stages of evolution, the energy at lower temperatures can only come from gravitational shrinkage. An epoch in such studies was reached when Cowling relieved anxieties as to how a star could remain stable at all under such extreme temperature-dependence of its energy sources.

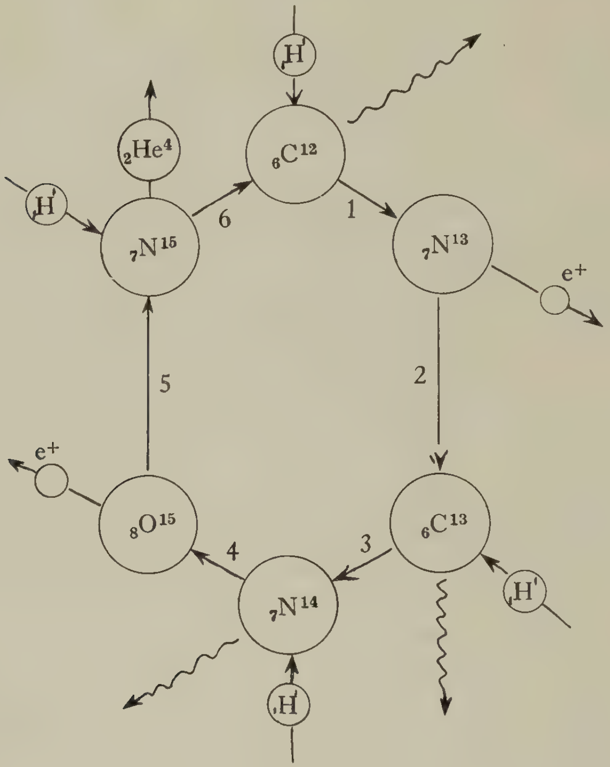

Fig. 3. Carbon-nitrogen cycle for production of helium from hydrogen.

It was realized very soon that the steep temperature coefficient of nuclear reactions must mean that the rate of consumption of hydrogen would be so excessive at the centre of the star that this region would rapidly deplete itself of fuel ; the zone of reaction and energy production must therefore migrate outwards, marking a growing radius capable of temperature rise and yet rich enough in hydrogen. So the ‘ point-source ’ of energy alters into a ‘ shell-source ’ as evolution of the star progresses, leaving a central core dead and isothermal (fig. 4). How far outwards, and down the original temperature gradient of the star, this zone can migrate by local heating, may be disputed ; but it is certain that gradients of density or any other parameter must have ceased for any but young stars to be describable by a single polytrope index expressing some single law of gradation.

Motz, Keller, Kushwaha and others have accordingly retained the usefulness of the index *n* by treating it as ‘ the fictitious index which if real would make the star rotate its apse at the same rate as actually observed ’, while recognizing that the star is composite. Indeed during much of its life the star is probably divided into three phases, a central isothermal core devoid of hydrogen, an envelope too cool for nuclear reaction, and an intermediate region of energy production where molecular weight is steeply altering while the boundary between hydrogen-rich and hydrogen-poor moves through the star as a rapid criterion of ageing. The expansion or contraction of local portions of the radius will occur as responsibility for escape from the region of energy-production is shifted from convection to specific absorption coefficients or to electron scattering.

These many physical parameters for denoting the stage of evolution of a star have decided the theoretical models devised, notably by the Princeton astronomers associated with Martin Schwarzschild ; papers by Sandage, Oke, Härm and Rogerson, as well as by Tayler in this country, provide through these models a possibility of quantitative prediction of apsidal motion constants k and the associated n, so long as it is remembered that only the ' effective ' or ' fictitious ' index is intended. We quoted earlier some equations for *observable* k, and will now quote without detailed development some comparable equations for the *theoretical* k ; this latter does not depend on observed data such as P'/P, but on the distortions of stellar models into which parameters denoting spread of the composite zone of hydrogen burning can be inserted as mark of age. As with the observed k, we quote from the earliest (Russell) and the latest (Schwarzschild) forms. In the former,

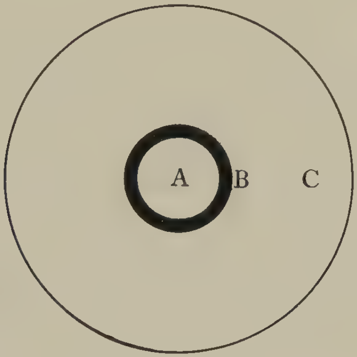

Fig. 4. Isothermal core (A), zone of energy production (B), non-reacting envelope (C), in a star.

$$k=\frac{e-\frac{1}{2}\phi}{\phi}=\frac{3-\eta_1}{2\eta_1+4}$$

where η_1 is the surface value of a quantity defined by

$$a\frac{d\eta}{da}+\eta^2-\eta-6+\frac{6\rho}{\bar{\rho}}(n+1)=0$$

and denoting the distortion of a sphere along radial distance a, in the formal dynamics of Radau in the nineteenth century. It is the natural equation for obtaining any quantity such as k dependent on the density ratio ρ_c/ρ_m which we recollect was Russell's way of expressing the degree of concentration within the star.

Passing over several of the historical and mathematically intricate steps, the latest equation for k (Schwarzschild 1958) is

$$k=\left\{\frac{3c_2-r\,dc_2/dr}{4c_2+2r\,dc_2/dr}\right\}.$$

This solves a recognizable descendant of the equation used by Russell, as indeed c and dc/dr come from integration of

$$\frac{d^2c_2}{dr^2}+\frac{2U}{r}\frac{dc_2}{dr}-2\frac{3-U}{r^2}c_2=0$$

This equation, like that of Russell, is a legacy from a Victorian dynamics of perturbations which its authors could not have envisaged as serving so startling a physics. The quantity U is a term which varies according to the model adopted for the stage of evolution by hydrogen-depletion in the stellar interior, and allows for the composite structure imposed across the zone of nuclear reaction separating core and envelope.

6. Evolutionary suggestions of Schwarzschild and Kushwaha

As example of the task of relating apsidal motion to evolutionary results of nuclear reaction, we will exhibit some recent attempts in the form published by Kushwaha (1957). His graph plots the effective polytrope index, more conveniently to our discussion than the similar graphical treatment of log k in the book by Schwarzschild (1958). The mathematical steps employ methods used earlier by Motz. For our purpose the graph has been adapted to abscissae denoting temperature, and is therefore not to exact scale (fig. 5).

The conclusions and suggestions from such a graph may be summarized as follows.

(i) Theoretical and observational values of the polytrope index n agree in order of magnitude.

(ii) Their slight rise, as temperature and mass of a selected star are reduced, is also common to theory and observation.

(iii) But although the observations scatter within these two rough agreements, they are all somewhat in excess of the theoretical values.

(iv) This excess is reduced by about half, if the models chosen for computing the theoretical k and n represent stars whose hydrogen depletion has proceeded to advanced stages of heterogeneity in the transition zone between isothermal core of star and its radiative envelope.

(v) The remaining excess has at present no explanation ; the closing of the gap, although in principle permitting deduction of the ages of individual stars by ‘ fitting ’ theoretical model to observed apsidal motion, will demand greatly enhanced precision both in observed data and in manipulation of the models.

We proceed finally to a wider critique, suggesting where the general physicist might find the situation precarious or excitingly hopeful.

7. Critique and prospects

In principle, this enterprise of employing the dynamics of gravitational orbits as a clue to the evolution of energy sources within a star, offers the unexpected and rare delight of combining the old and the new in scientific history. But the number of steps in argument which we have been retailing leaves a query as to whether some *tour de force* has been displayed with little right to expect a reliable conclusion ; such a query must be faced in any general exposition, though out of place in any of the researches themselves. The following seem relevant points to raise.

(i) Even with the geometrical devices of de Kort, the great length of the apsidal period compared with any occasion of observation creates suspicion that something other than apse motion may be contributing to the observed fluctuation in timing the eclipses ; for example, perturbation by a third and hidden star, or some instability or the recoil from local giant eruptions. It is not surprising that so few binaries passed the scrutiny of Keller and Limber, and it is a sobering experience to read in detail the great dossier of eclipsing binaries by Kopal and Shapley, where ' suspected of apse rotation ' is often the only sound decision.

(ii) The comparison of theory and observation in fig. 5 has had to rely not only on so few stars, but on stars unevenly distributed along the main classification known to astrophysics. The most detailed study of mutual gaseous interaction between binary components, by Kopal (1959), has provided the fullest data about cool stars of low density which associate with smaller hotter companions : but these the accepted equations of apse motion cannot adequately handle.

(iii) Those studies by Kopal, of the gases drawn out of a star by proximity of its companion, have revealed discrepancies with the evolutionary line of development calculable for a single isolated star. Some of these divergences due to binary interference may conceivably contribute to the ' gap ' of fig. 5, the difference between observed and theoretical points being due not only to age but to the fact that theory has so far only handled single stars and then been transferred unaltered to the case of a binary.

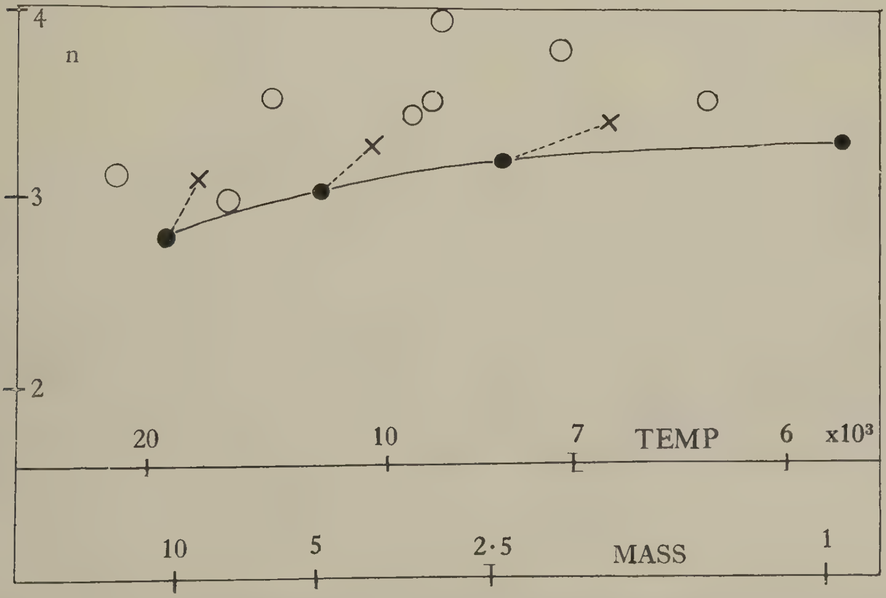

Fig. 5. Effective polytrope index (n) for stars of given surface temperature and multiple of solar mass (*after Kushwaha*). ○ from observed apse motion ; ● theory for young star ; × theory for older star.

(iv) A brief paper by Plavec in the Liége Astrophysical Symposium (1958) initiated the difficult problem of instability expected when we abandon the usual assumption of equal period in spin and orbital revolution. Again we have to refer to Kopal for the modifications which would accompany such a multiplication of the periodicities in tidal distortion. The considerations of (ii) and (iv) allow the apse parameter ϕ to show violent local changes.

(v) The assumption underlying the stellar models of an advancing shell of nuclear reaction, on which the inferences as to age from apse motion are based, is only valid if diffusion and centrifugal circulation of gas in the star is so slow that ‘ mixing ’ can be neglected, and the lost hydrogen never replenished from the outlying regions. Papers by Sweet and by Mestel in this country have already faced this very difficult problem of rotational mixing, which is also complicated by the fact that the highly ionized gases are moving in magnetic fields. Schwarzschild, in the book which we have quoted, considers the mixing is usually slight, but admits that in very hot and fast spinning stars it might become important.

Physicists anxious to appreciate critically this rare chance of associating the dynamics of Kepler orbits with a problem of nuclear reactions, may well regard future progress in the topic as indicated by the following remarks which I owe to a personal letter from Professor Kopal : "All deductions relating the observed rate of apsidal motion with any effective polytrope index involve the following tacit assumptions, none of which need be true : namely, that each component rotates like a rigid body, that its axis of rotation is perpendicular to the orbital plane, and that (in the case of tidal distortion) the effects of tidal lag (i.e. of stellar viscosity) are negligible. In addition many authors have assumed that each component rotates about its axis with the Keplerian angular velocity—which is contradicted in several cases by the spectroscopic observational evidence (line profile broadening, rotational effect), revealing that in systems characterized by strongly eccentric orbits the components rotate as a rule faster than they revolve."

Selected references with comment

Apse migration is mentioned too briefly in the astronomical text books and too complicatedly in the dynamical treatises. Rare exceptions are in *Astronomy*, Vol. 1, by Russell, Dugan, and Stewart (relating to instances in the Solar system), and in *Story of Variable Stars*, by Campbell and Jacchia. The earliest research paper on the binary star case is very readable, H. N. Russell, *Monthly Notices, Roy. Astron. Soc.*, 1928, **88,** 641. The final dynamical treatment, before nuclear reaction came into the problem, is in T. E. Sterne, M.N.R.A.S., 1939, **99,** 451, 662, 670. The present thermonuclear problem in apse motion is discussed in M. Schwarzschild, *Structure and Evolution of the Stars*, 1958. Of the many post-war papers, that by Keller and Limber, *Astrophys. Journ.*, 1951, **113,** 637, is exceptional for detail of observational precision. The most ambitious attempt at nuclear correlation is in Kushwaha, ApJ., 1957, **125,** 242. The relation to period fluctuation in general is discussed in Z. Kopal, *Close Binary Systems*, 1959.

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Diffraction in Three Dimensions

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1. Introduction

There seems to be a certain amount of confusion—both in the literature and in the minds of students—concerning diffraction by one, two and three dimensional objects. Most optical-diffraction studies are concerned with one- or two-dimensional objects and it is natural that these should be grouped together; the experimental arrangements are simple and well known to physicists. The most familiar example of diffraction from a *three*-dimensional object, however, is that of x-rays by a crystal and for this the experimental arrangement is much more complicated; the film is usually cylindrical and completely surrounds the crystal, the crystal must be rotated or moved in some other way, and many exposures may be necessary to record the complete pattern. The experimental and theoretical problems in this field are, in fact, so much more complex that x-ray crystallography is regarded by many physicists as a specialist subject completely outside their normal range of study. The unfortunate consequence is that the whole subject of diffraction by three-dimensional objects tends to be regarded as the special province of the mathematical crystallographer. Recently attempts have been made, with some success, to use the principles of optical diffraction in solving x-ray problems and in the process a great deal has been learnt about the underlying physics of x-ray diffraction. It is one of the purposes of the present paper to suggest that it may now be the turn of the special ideas and concepts developed for x-ray studies to help in deepening our understanding of optical problems.

2. Preliminary ideas

In studying diffraction processes it is essential to bear in mind that three-dimensional waves in a three-dimensional medium are involved. Even when plane waves are incident on a plane object of finite size the wave fronts immediately after impinging on the object—regardless of its shape—may only be described in terms of three dimensions. Diffraction is, by its very nature, a three-dimensional process involving *angles*, diffraction patterns can therefore only be described satisfactorily in terms of angles; the patterns usually observed arise from the intersection by a plane (the screen or film) of sets of waves proceeding in various directions in space. (Misconceptions may arise very early in the training of a student because of the widespread use of slit sources in diffraction demonstrations. Slits are used at an early stage partly *because* they obscure the three-dimensional character of diffraction and so do not raise difficulties when the subject is first presented. It is important, however, that the students' attention should be drawn to this simplification by showing patterns produced with point sources as well).

The crystallographer handles his three-dimensional diffraction patterns in many different ways, but they are all based, implicitly or explicitly, on the concept of ‘ reciprocal space ’ or, mathematically speaking, on Fourier transformation. These terms often seem disconcerting—particularly to nonmathematicians—but it is possible to introduce them without the need for very complicated mathematics. Although shortage of space prohibits anything like a full discussion here, an attempt will be made to show the lines along which such an introduction could proceed.

3. Introduction to reciprocal space

Objects which are very small compared with the wavelength of the radiation incident on them scatter radiation more or less uniformly as though they were new sources. For larger objects phase changes are introduced in the radiation scattered from different parts of the object; these phase changes depend both on the directions of incidence and of observation. The *larger* the object is compared with the wavelength, the *smaller* will be the angular change necessary to produce a given phase difference. This point is worth considerable attention as it is the origin of the reciprocal relationships between objects and their diffraction patterns. It may be helpful to illustrate it by means of moiré patterns. In fig. 1 strips of lined transparent material (manufactured for 'shading' drawings for reproduction) have been used to represent the crests of waves travelling from different points on an object; the moiré fringes show clearly the lines along which the beams are in phase with each other. The illustration is not perfect of course since only the coincidence of *peaks* may be represented; no representation of the coincidence of peaks and troughs, which would lead to zero observed amplitude, is possible. Also in practice the wave

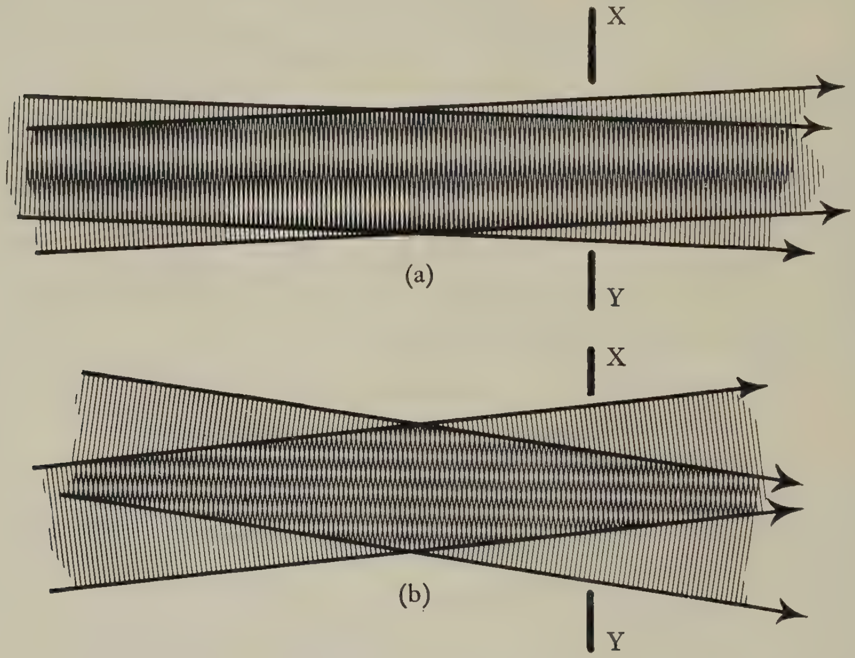

Fig. 1 (*a*). The fine lines represent the crests of waves travelling from two points on an object which are rather close together; the moiré fringes are widely spaced and show that the phase difference between the waves changes slowly along a line such as XY.

(*b*). If the two points are *further apart* the moiré fringes are *closer together* and the phase difference changes more rapidly along XY.

fronts would be spherical, though at a large distance they approximate to planes. In spite of these defects, however, it can be seen clearly that in fig. 1 (*a*), where the waves are travelling from two points which are close together, the phase difference changes slowly as one proceeds along a line such as XY so that maxima are wide apart; in fig. 1 (*b*), where the waves are travelling from two points which are further apart, the maxima are closer together. An even more striking demonstration may be given if two pieces of the lined material are pivoted at points representing scattering centres so that the angles may be varied continuously.

4. Consideration of the scattering process in more detail

Consider two points A and B (fig. 2) on which a parallel beam of radiation of wavelength λ is incident. AB is not necessarily perpendicular to the incident direction and hence the waves reaching A and B may have a phase difference. Let us now suppose that A and B act as scattering centres without introducing further change of phase (or with *identical* phase changes) and then consider the final phase difference between waves scattered in some other direction. (The figure is intended to be three-dimensional; that is, the directions of incidence and emergence and AB are not necessarily coplanar.) The total path difference introduced is

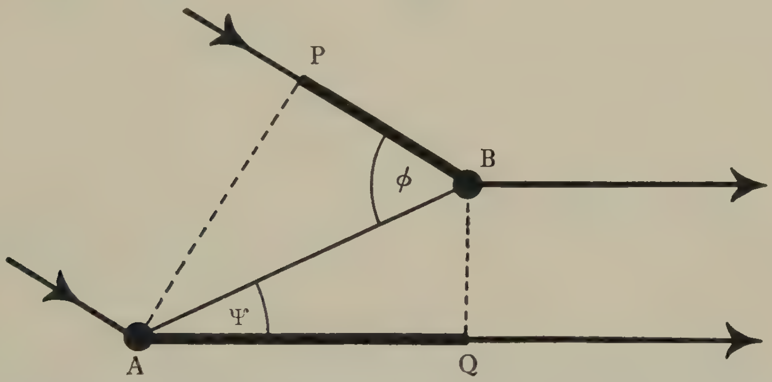

Fig. 2. The path difference for plane waves incident at an angle ϕ with AB and emergent at an angle ψ is the difference between the lengths of the two bold lines.

$$AQ - BP = AB\ [\cos\psi - \cos\phi]$$

and the total phase difference δ is therefore $(2\pi/\lambda)$ AB $[\cos\psi - \cos\phi]$. It is important to remember that, since the figure is three-dimensional ϕ and ψ do not define specific directions in space relative to AB; they each define cones of directions obtained by rotating AQ or BP around AB as axis.

Now $\cos\psi$ and $\cos\phi$ may both range from $+1$ to -1 and hence the bracketed quantity may take any value between $+2$ and -2. A given value of the bracket may thus arise from various combinations of ψ and ϕ. For example, $[\cos\psi - \cos\phi]$ is zero if $\psi = \phi$, that is, for a whole range of *pairs* of values of ϕ and ψ. On the other hand $[\cos\psi - \cos\phi]$ is equal to $+2$ *only* when $\psi = 0$ and $\phi = 180°$. This kind of consideration suggests that it may not be necessary to include *all* directions of incidence and observation in order to give a complete description of the scattering properties of an object; some degree of ‘ condensation ’ in terms of the bracketed quantity may be possible. The use of vector methods makes the achievement of this condensation quite direct (see, for example, Lipson & Taylor, 1958), but it is sometimes helpful to demonstrate the result without using vectors. Let us treat first of all the case of normal incidence ($\phi = 90°$) for which δ is $(2\pi/\lambda)$ AB $\cos\psi$ and attempt to construct a three-dimensional graph which would show the phase difference δ for all possible directions of observation. Consider fig. 3 in which OR is a line of length $1/\lambda$ and represents the direction of observation. OO′ represents the direction of incidence. (In order to draw this figure it is, of course, necessary to choose some suitable scale; for example, 10 cm to represent $1/5000\text{Å}^{-1}$ would be suitable for visible light). OR may rotate out of the plane of the paper and R may thus lie anywhere on the surface of a sphere of radius $1/\lambda$. The values of δ could thus be plotted on the surface of the sphere to give us the required graph.

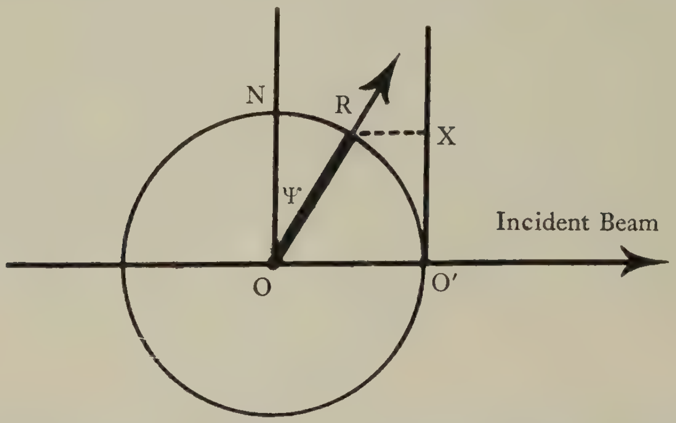

Fig. 3. The bold line is of length $1/\lambda$ in the direction of observation; as the direction of observation changes the point R remains on the surface of a sphere—the sphere of observation.

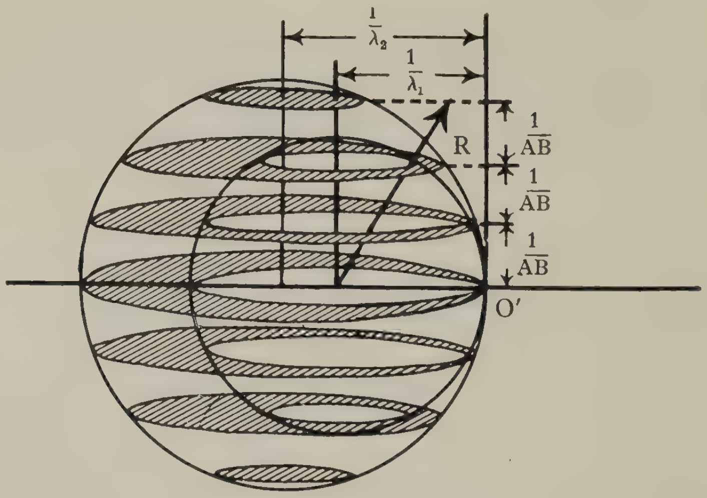

Fig. 4. The smaller sphere is the sphere of observation from fig. 3, and the horizontal circles on its surface represent points for which $\delta = 2n\pi$; the larger sphere corresponds to a smaller wavelength (radius $= 1/\lambda$) and shows how the circles develop into planes.

A moment's consideration will show that δ is the same for all points on the surface of the sphere having the same value of ψ; the graph is thus a set of circles on the surface of the sphere having centres on ON and their planes perpendicular to ON. Fig. 4 shows the set for which $\delta = 2n\pi$, that is, it corresponds to those directions for which the beams are in phase after scattering. One circle is equatorial $[n = 0]$ and the rest are such that

$$(2\pi/\lambda)AB \cos\psi = 2n\pi.$$

That is,

$$\cos\psi/\lambda = n/AB,$$

$$\text{but } OR = 1/\lambda$$

and hence OX, the projection of OR on a perpendicular at O′, is given by

$$OX = OR \cos\psi = \cos\psi/\lambda = n/AB.$$

Thus the planes of the circles for which δ is $2n\pi$ are separated by perpendicular distances $1/AB$. In other words, their separation is the *reciprocal of the separation of the scattering points in real space.* If the wavelength were changed, the sphere diameter would change but the circles would remain in the same planes ; fig. 4 also illustrates this point. Since the beams add vectorially the amplitude for directions corresponding to the circles would be a maximum, and it can be seen that it would vary sinusoidally in between. The scattering properties of the two points in real space, for normal incidence and any wavelength, could thus be summarised by constructing a three-dimensional figure consisting of horizontal planes perpendicular to the line joining the points, varying sinusoidally in density and with maxima separated by a distance reciprocal to the separation of the two points. The amplitude of scattering in a particular direction is then the ' density ' of the solid at a point found by drawing a line of length $1/\lambda$ from O in the direction of observation. It is clear that, for one wavelength and one direction of incidence, only points of the solid lying on the surface of the sphere may be observed.

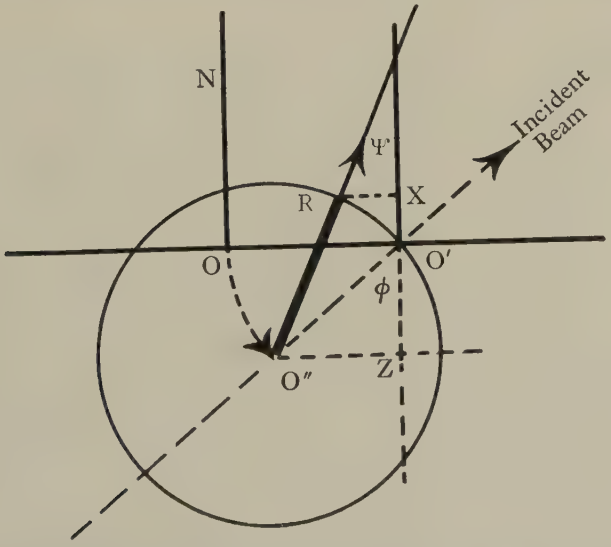

Fig. 5. When the incident beam is not normal to AB, fig. 3 must be modified; the centre of the sphere of observation rotates from O to O″ about O′. Here O′X is OR($\cos\psi - \cos\phi$), and this has the same significance as O′X in fig. 3 for normal incidence.

This idea—the use of the so-called ‘ reciprocal solid ’ (Lipson & Taylor 1958)—may be extended to other angles of incidence and to more complicated objects. Lack of space forbids a complete discussion of these extensions, but fig. 5 indicates the way in which the idea may be developed. It turns out that the same solid figure may be used again, but the sphere is now rotated about O′. If all possible directions of incidence are considered, a sphere of radius $2/\lambda$ is swept out and no information about the reciprocal solid outside this sphere may be obtained. The concept of ‘ limit of resolution ’ is of course closely tied with this limitation. More complicated objects may be considered by adopting some point as an arbitrary origin, considering every other point in turn relative to this origin, and adding all the results. The final solid depends only on the object; it is, mathematically speaking, a graphic representation of the Fourier transform of the object.

5. APPLICATION OF THE RECIPROCAL-SOLID CONCEPT IN COMPARING X-RAY AND OPTICAL DIFFRACTION

The problem outlined in section 2—the difference in experimental procedure for observing optical and X-ray diffraction—may now be considered again. A typical optical-diffraction object might be a pair of slits, each 0·002 cm wide spaced 0·01 cm apart; the corresponding reciprocal solid would have maxima 100 cm^{-1} apart and the radius of the sphere $[1/\lambda]$ for sodium light is approximately 17 000 cm^{-1} [In X-ray literature this is called the sphere of reflexion but for our present purpose the term ‘ sphere of observation ’ is more appropriate]. The sphere is thus so large that the part which intersects the interesting region of the reciprocal solid—about 10 maxima—is practically planar and perpendicular to OO′. The diffraction pattern observed with *one* direction of incidence and a small range of angles of observation is more or less a plane section of the reciprocal solid.

The corresponding X-ray object [obviously hypothetical!] would be two atoms spaced, say, 2×10^{-8} cm apart; the reciprocal solid would have planes 5×10^{7} cm^{-1} apart. The sphere for CuKα radiation has a radius of about 7×10^{7} cm^{-1}, so only 1 or 2 maxima would be observable and the section of the solid would be far from planar. A more practical object would be a complete crystal which is periodic in three dimensions. The reciprocal solid then turns out to be a set of isolated points arranged regularly in space (the ‘ reciprocal lattice ’ of X-ray crystallography), but the relative dimensions of the interesting region would still be the same. Clearly the chance that the sphere of observation passes through very many of the points simultaneously is very remote. It is therefore necessary to vary the direction of incidence to make the sphere pass through the points and permit their observation; fig. 6 is an illustration of this process in two dimensions. Because the wavelength is comparable with the interesting dimensions, a non-planar section of the reciprocal solid is involved; because a non-planar section is involved the pattern is distorted and not obviously a regular lattice when first recorded; because the crystal is periodic in three dimensions the reciprocal solid reduces to points in space and the spherical section is unlikely to include more than one or two points unless the direction of incidence is varied.

The important conclusion from this discussion is that the differences between x-ray and optical diffraction occur primarily because the ratio of interesting dimensions to wavelength is so very different, rather than because of the three-dimensional nature of the crystal lattice and the two-dimensional nature of the diffracting object for light.

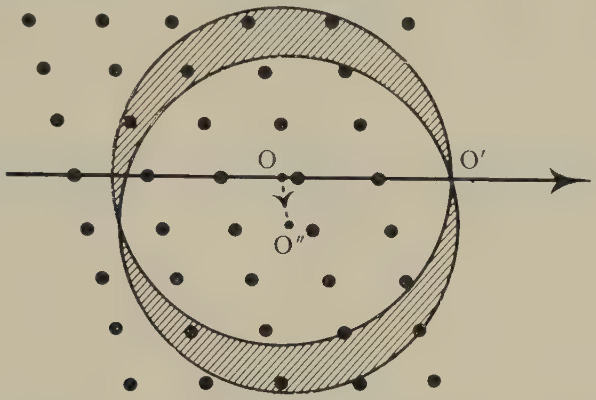

Fig. 6. A two-dimensional representation of the reciprocal solid for a crystal showing the sphere of observation in two different positions. In neither position does it pass accurately through more than one point; the shaded portion shows the points through which the sphere would pass in moving from one position to the other.

6. APPLICATION OF THE RECIPROCAL-SOLID CONCEPT TO THE MICHELSON INTERFEROMETER

For the purpose of the present discussion the Michelson interferometer—see, e.g., Ditchburn (1952)—may be regarded as a means of combining two coherent beams of light, one from a fixed object and the other from an object whose position in space can be varied. [Both objects are of course spatial images.] Operation of the lead screw provides longitudinal relative motion of the objects, and lateral relative movement is provided by tilting one of the mirrors.

Let us consider first of all what happens when the mirrors are parallel and their position is adjusted so that the images lie both on the axis and very close to each other. The reciprocal solid (as in section 4) consists of parallel planes perpendicular to the axis and with spacings reciprocally related to the spacing of the two images. For a given angle of incidence—in this case parallel to the axis—the sphere of observation obviously cuts these planes in concentric circles; if the spacing of the images is increased, the spacing of the planes of the reciprocal solid decreases and the circles become closer together. If one mirror is then tilted the line joining the images swings away from the axis, the reciprocal solid thus rotates and the sphere of observation now cuts the planes in circles whose centre lies off the axis—the familiar curved fringes. If adjustments are now made so that the images are side by side, the solid swings completely round so that the planes are parallel to the axis and the intersection with the sphere of observation is now a set of parallel vertical lines—the straight fringes. Figure 7 illustrates this application of the concept.

This treatment thus helps to emphasize the relationships between the various possible types of fringes.

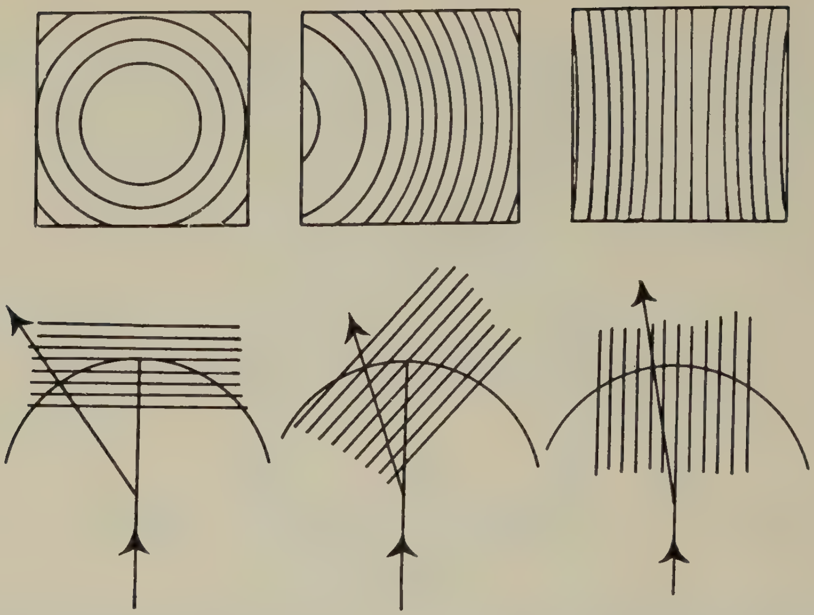

Fig. 7. The upper figures illustrate three different kinds of fringes obtainable with a Michelson interferometer; the lower figures are horizontal sections of the corresponding reciprocal solids. Portions of the spheres of observation are shown, and arrows indicate the direction of observation for the second maximum to the left of the centre. (Both the fringes and the solid are, of course, sinusoidal; the lines in this diagram represent maxima.)

7. Conclusions

In such a short article it would be impossible to explore the full implications of the use of the reciprocal-solid idea in optical diffraction studies, but it is hoped that sufficient indication of its usefulness has been given. It would obviously be undesirable to teach diffraction from the start in terms of the solid, though some of the ideas might be introduced at a fairly early stage. The important point for introductory teaching is to make clear the simplifications involved in the more usual treatments and to implant the idea that *all* diffraction phenomena are three-dimensional. In this way the ground may be prepared for the fascinating idea of an essential unifying principle underlying all diffraction processes.

This idea of unity has long been available for the mathematically minded through the concept of Fourier transformation; it is important to make it available also to those who, like the author, find a physical approach more understandable.

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Electromagnetism as a Second-order Effect

(II) The Electric and Magnetic Fields of a charge moving with uniform velocity

by W. G. V. ROSSER

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SUMMARY

The Electric and Magnetic Fields of a charge moving with uniform velocity are derived from Coulomb's law taking the transformations of the Special Theory of Relativity as axiomatic.

1. INTRODUCTION

In Paper I (Rosser 1959) it was shown that electromagnetism was a second order effect and it was illustrated how in two simple cases the magnetic forces between moving charges can be calculated from Coulomb's law, if the force transformations of the theory of special relativity and the invariance of total electric charge are taken as axiomatic. In this paper we proceed to consider more general cases. It is shown in many text-books on Relativity that electromagnetic forces can be obtained from Coulomb's law, but almost invariably it is proved by using the transformations for either the potentials Φ and **A**, or the electric intensity **E** and magnetic induction **B**. In this paper we will use the transformation formulae for force, leaving the introduction of **E** and **B** until after the force between uniformly moving charges has been calculated.

2. SUMMARY OF THE TRANSFORMATION FORMULAE OF SPECIAL RELATIVITY

Consider two inertial frames Σ and Σ', with Σ' moving with a uniform velocity v relative to Σ along the positive OX axis. We will take the Lorentz transformations connecting the coordinates x', y', z', t' in Σ' with x, y, z, t in Σ as axiomatic, namely

$$\begin{aligned} x' &= \frac{(x-vt)}{\sqrt{(1-v^2/c^2)}} \\ y' &= y \\ z' &= z \\ t' &= \frac{\{t-(vx)/c^2\}}{\sqrt{(1-v^2/c^2)}} \end{aligned} \tag{2.1}$$

The inverse transformations are always obtained by replacing x by x' etc. and v by $-v$.

If the velocity of a particle in Σ is $\mathbf{u}$ having components u_x, u_y and u_z, then in Σ' we have

$$\begin{aligned} u_x' &= \frac{dx'}{dt'} = \frac{dx - v\,.\,dt}{dt - v/c^2\,.\,dx} = \frac{(u_x - v),}{\{1-(v\,.\,u_x)/c^2\}} \\ u_y' &= \frac{dy'}{dt'} = \frac{u_y\,.\,\sqrt{(1-v^2/c^2)}}{\{1-(v\,.\,u_x)/c^2\}} \\ u_z' &= \frac{dz'}{dt'} = \frac{u_z\,.\,\sqrt{(1-v^2/c^2)}}{\{1-(v\,.\,u_x)/c^2\}}\,. \end{aligned} \tag{2.2}$$

The assumption of the conservation of momentum in elastic collisions in all inertial frames of reference necessitates a redefinition of mass, namely in Σ we have

$$m=\frac{m_0}{\sqrt{(1-u^2/c^2)}} \tag{2.3}$$

where m_0 is the rest mass of the particle, and u its velocity measured in Σ, is given by

$$u^2=u_x{}^2+u_y{}^2+u_z{}^2.$$

In Σ' we have

$$m'=\frac{m_0}{\sqrt{(1-u'^2/c^2)}} \tag{2.4}$$

where u' is the velocity of the particle measured in Σ' and is given by

$$u'^2=u_x{}'^2+u_y{}'^2+u_z{}'^2.$$

In Σ the force acting on a particle is defined as

$$\begin{aligned} F_x&=\frac{\mathrm{d}}{\mathrm{d}t}(m\,.\,u_x)\\ F_y&=\frac{\mathrm{d}}{\mathrm{d}t}(m\,.\,u_y)\\ F_z&=\frac{\mathrm{d}}{\mathrm{d}t}(m\,.\,u_z). \end{aligned} \tag{2.5}$$

In Σ' the force is defined as

$$F_x{}'=\frac{\mathrm{d}}{\mathrm{d}t'}(m'\,.\,u_x{}')\text{etc.} \tag{2.6}$$

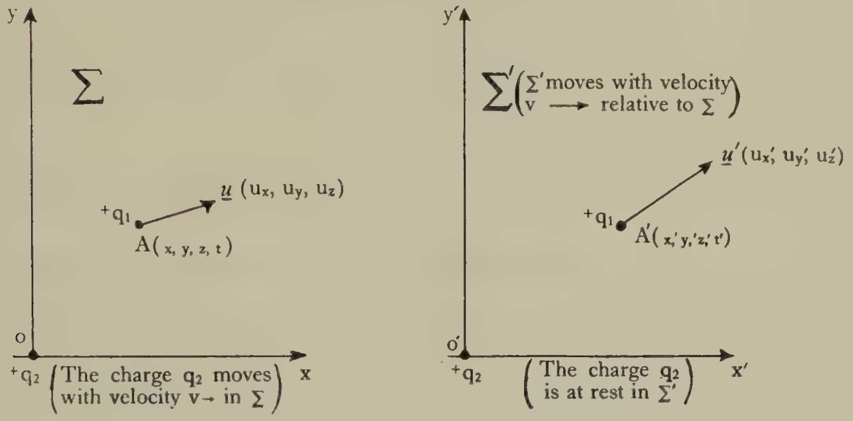
Substituting for m, u_x, u_y, u_z etc. in eqns. (2.5) we obtain the following formulae for the transformation of force

$$\begin{aligned} F_x&=F_x{}'+\frac{u_y{}'\,.\,v}{(c^2+u_x{}'\,.\,v)}\,.\,F_y{}'+\frac{u_z{}'\,.\,v}{(c^2+u_x{}'\,.\,v)}\,.\,F_z{}'\\ F_y&=\frac{c^2\sqrt{(1-v^2/c^2)}}{(c^2+u_x{}'\,.\,v)}\,.\,F_y{}'\\ F_z&=\frac{c^2\sqrt{(1-v^2/c^2)}}{(c^2+u_x{}'\,.\,v)}\,.\,F_z{}'. \end{aligned} \tag{2.7}$$

The calculation is straightforward, but the algebra is tedious. The derivation is outlined in the Appendix.

3. THE FORCE BETWEEN CHARGES MOVING WITH UNIFORM VELOCITIES IN EMPTY SPACE

Using the transformations given in § 2 we now derive, from Coulomb's law, the force between charges moving with uniform velocity assuming the invariance of electric charge. In Σ at a given time $t=0$ consider a charge q_2 at the origin moving with a uniform velocity v along the OX axis and a charge q_1 at a point x, y, z, moving with a velocity $\boldsymbol{u}$ having components u_x, u_y and u_z as shown in fig. 1 *a*. Consider a frame Σ' moving to the right with a uniform velocity v relative to Σ along the OX axis as shown in fig. 1 *b*. Let the origins coincide at $t=0$. The charge q_2 remains at rest at the origin in Σ'. We want to calculate the force on q_1 due to q_2 measured in Σ at the time $t=0$ when q_2 is at the origin.


y
Σ
$+q_1$
$\underline{u}\ (u_x, u_y, u_z)$
A(x, y, z, t)
O
$+q_2$
(The charge q_2 moves with velocity v→ in Σ)
x
y′
Σ′ (Σ′ moves with velocity v → relative to Σ)
$\underline{u}'(u_x', u_y', u_z')$
$+q_1$
A′(x′, y′, z′, t′)
O′
$+q_2$
(The charge q_2 is at rest in Σ′)
x′

(a) (b)

Fig. 1. In the inertial frame Σ shown in Fig. 1 (*a*) both charges are moving with uniform velocity. In the inertial frame Σ' which moves along the OX axis with a velocity v relative to Σ, the charge $+q_2$ is at rest at the origin at all times.

From eqns. (2.1) if $t=0$ at all points in Σ, we have

$$\begin{aligned} x' &= \frac{x}{\sqrt{(1-v^2/c^2)}} \\ y' &= y \\ z' &= z \\ t' &= \frac{(-v\,.\,x/c^2)}{\sqrt{(1-v^2/c^2)}}. \end{aligned} \tag{3.1}$$

Hence in fig. 1 *b*

$$\mathrm{O'A'} = \sqrt{(x'^2+y'^2+z'^2)} = \sqrt{\left[\frac{x^2}{(1-v^2/c^2)}+y^2+z^2\right]} = \frac{s}{\sqrt{(1-v^2/c^2)}}$$

where

$$s = \sqrt{\{x^2+(1-v^2/c^2)(y^2+z^2)\}}. \tag{3.2}$$

In Σ', since the charge q_2 is always at rest at the origin, we assume that the force on q_1 due to q_2 measured at the point A′ in Σ' at the time

$$t' = \frac{(-v\,.\,x)/c^2}{\sqrt{(1-v^2/c^2)}}$$

(corresponding to $t=0$ in Σ) is given by Coulomb's law, namely

$$F' = \frac{q_1 q_2}{\mathrm{O'A'}^2}.$$

Writing out the components of this force we have:—

$$F_x' = \frac{q_1 q_2}{\mathrm{O'A'}^2} \cdot \frac{x'}{\mathrm{O'A'}}. \qquad \text{etc.}$$

Substituting for x' and O′A′ we have

$$F_x' = \frac{q_1 q_2}{s^3}\,.\,x\,.\,(1-v^2/c^2).$$

Similarly

$$F_y' = \frac{q_1 q_2}{s^3} \,.\, y \,.\, (1 - v^2/c^2)^{3/2} \tag{3.3}$$

and

$$F_z' = \frac{q_1 q_2}{s^3} \,.\, z \,.\, (1 - v^2/c^2)^{3/2}.$$

From equations (2.7) we have

$$F_x = F_x' + \frac{u_y' \,.\, v}{c^2 + u_x' \,.\, v} \,.\, F_y' + \frac{u_z' \,.\, v}{c^2 + u_x' \,.\, v} \,.\, F_z'.$$

Substituting for F_x', F_y' and F_z' from eqns. (3.3) and for u_x', u_y' and u_z' from eqns. (2.2) we have

$$\begin{aligned} F_x = {} & \frac{q_1 q_2}{s^3} \,.\, x \,.\, (1 - v^2/c^2) \\ & + \frac{u_y \sqrt{(1 - v^2/c^2)}}{\{1 - (v.u_x)/c^2\}} \,.\, \frac{v}{\left[c^2 + \dfrac{v(u_x - v)}{\{1 - (v.u_x)/c^2\}}\right]} \, \frac{q_1 q_2}{s^3} \,.\, y \,.\, (1 - v^2/c^2)^{3/2} \\ & \qquad + \frac{u_z' \,.\, v}{(c^2 + u_x' \,.\, v)} \,.\, F_z'. \end{aligned}$$

The second term on the right-hand side is

$$u_y \,.\, \frac{(1 - v^2/c^2)^2}{\{1 - (v \,.\, u_x)/c^2\}} \,.\, v \, \frac{\{1 - (v \,.\, u_x)/c^2\}}{(c^2 - v^2)} \,.\, \frac{q_1 q_2}{s^3} \,.\, y$$

$$= \frac{q_1 q_2}{s^3} \,.\, y \,.\, u_y \,.\, v/c^2 \,.\, (1 - v^2/c^2).$$

Similarly the third term is

$$\frac{q_1 q_2}{s^3} \,.\, z \,.\, u_z \,.\, v/c^2 \,.\, (1 - v^2/c^2).$$

Hence

$$F_x = \frac{q_1 q_2}{s^3} (1 - v^2/c^2)[x + v/c^2(y \,.\, u_y + z \,.\, u_z)] \tag{3.4}$$

From eqns. (2.7) we have

$$F_y = \frac{c^2 \sqrt{(1 - v^2/c^2)}}{(c^2 + u_x' \,.\, v)} \,.\, F_y'.$$

Substituting for F_y' from eqns. (3.3) and for u_x' from eqns. (2.2) we have

$$F_y = \frac{c^2 \sqrt{(1 - v^2/c^2)}}{\left[c^2 + \dfrac{(u_x - v)}{\{1 - (v \,.\, u_x)/c^2\}} \,.\, v\right]} \,.\, \frac{q_1 q_2}{s^3} \,.\, y \,.\, (1 - v^2/c^2)^{3/2}.$$

Now

$$1 + \frac{(u_x - v) \,.\, v}{c^2\{1 - (u_x \,.\, v)/c^2\}} = \frac{\{1 - (u_x \,.\, v)/c^2 + (u_x \,.\, v)/c^2 - v^2/c^2\}}{\{1 - (u_x \,.\, v)/c^2\}} = \frac{(1 - v^2/c^2)}{\{1 - (u_x \,.\, v)/c^2\}}.$$

Hence

$$F_y = \frac{q_1 q_2}{s^3} \,.\, (1 - v^2/c^2)\{1 - (v \,.\, u_x)/c^2\} \,.\, y. \tag{3.5}$$

Similarly

$$F_z = \frac{q_1 q_2}{s^3}(1 - v^2/c^2)\{1 - (v \,.\, u_x)/c^2\} \,.\, z. \tag{3.6}$$

Equations 3.4, 3.5 and 3.6 were obtained using the force transformations by Tolman (1913).

4. THE ELECTRIC AND MAGNETIC FIELDS DUE TO A CHARGE MOVING WITH UNIFORM VELOCITY

The electric intensity $\mathbf{E}$ and the magnetic induction $\mathbf{B}$ at a point are generally defined in terms of the Lorentz force acting on a test charge placed at the point; for example if $\mathbf{E}_2$ and $\mathbf{B}_2$ are the electric intensity and magnetic induction at A due to the charge q_2 at the origin in the inertial frame Σ as shown in fig. 1 *a*, then the Lorentz force on the charge q_1 (which we are now considering as our test charge) expressed in terms of the fields is given by

$$\mathbf{F} = q_1\mathbf{E}_2 + (q_1/c) \,.\, \mathbf{u} \times \mathbf{B}_2 = \mathbf{F}_{ELEC.} + \mathbf{F}_{MAG.}, \tag{4.1}$$

where q_1 is in e.s.u. and c is the velocity of light. We shall show later that eqns. (3.4), (3.5) and (3.6) can be expressed in this way.

The first term on the right-hand side is called the electric force (denoted $\mathbf{F}_{ELEC.}$) and the second term is called the magnetic force (denoted $\mathbf{F}_{MAG.}$). If the test charge q_1 were at rest in Σ then the only force measured would be $\mathbf{F}_{ELEC.}$ and E_2 could be defined by the relation

$$\mathbf{E}_2 = \frac{\mathbf{F}_{ELEC.}}{q_1}. \tag{4.1 a}$$

It is assumed that q_1 is so small that the effect of the induced charges due to q_1 can be neglected.

If the test charge q_1 moves with velocity $\mathbf{u}$ then, according to eq. (4.1), there is an extra contribution to the force (namely $F_{MAG.}$) over and above the electric force, given by

$$\mathbf{F}_{MAG.} = q_1/c \; \mathbf{u} \times \mathbf{B}_2.$$

The magnitude of this force is proportional to the velocity of the test charge. It depends also on the direction of $\mathbf{u}$. It is a maximum when $\mathbf{u}$ is perpendicular to $\mathbf{B}$ and a minimum when $\mathbf{u}$ is parallel to $\mathbf{B}$. We may thus define the direction of $\mathbf{B}$ as the direction in which the test charge is moving when it experiences no magnetic force. The strength of the magnetic induction is defined in terms of the maximum magnetic force, which is obtained when $\mathbf{u}$ is perpendicular to $\mathbf{B}$, that is

$$B = \frac{(F_{MAG.})_{MAX.}}{(q_1/c) \,.\, u}. \tag{4.1 b}$$

The sense of $\mathbf{B}$ is defined such that $\mathbf{F}_{MAG.}$, $\mathbf{B}$ and $\mathbf{u}$ obey the usual left-hand rule with the thumb of the left hand in the direction of $\mathbf{F}_{MAG.}$, the first finger in the direction of $\mathbf{B}$ and the second in the direction of $\mathbf{u}$ (for a posititive charge). This sense is in agreement with the direction in which the north ‘ pole ’ of a compass needle would point in empty space.

To simplify the geometry we put the test charge q_1 in the xy plane as shown in fig. 2, that is we make $z=0$ though u_z is not necessarily zero.

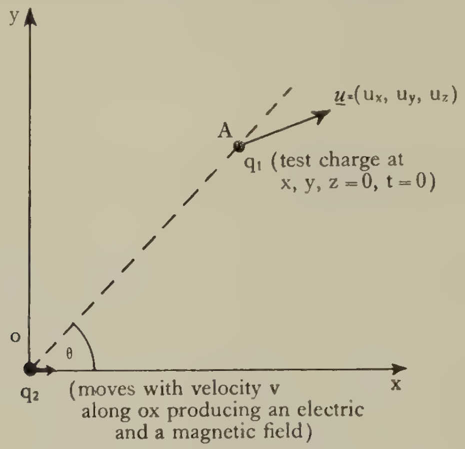


Fig. 2. Simplified form of fig. 1 *a*. The charge q_2 is assumed to produce an electric and a magnetic field. We are measuring the fields in Σ by means of the forces on the test charge q_1 placed at A in the xy plane at the instant q_2 is at the origin (i.e. $t=0$).

Writing the Lorentz force eqn. (4.1) in components we have

$$F_x = q_1E_x + q_1/c(u_y \,.\, B_z - u_z \,.\, B_y).$$

Comparing this with the equation

$$F_x = \frac{q_1q_2}{s^3}(1-v^2/c^2)(x+v/c^2 \,.\, y \,.\, u_y)$$

obtained by putting $z=0$ in eqn. (3.4), that is

$$F_x = q_1 \,.\, \frac{q_2}{s^3}(1-v^2/c^2) \,.\, x + \frac{q_1}{c} \,.\, u_y \,.\, \frac{q_2}{s^3}(1-v^2/c^2) \,.\, v/c \,.\, y \,.$$

we conclude that

$$E_x = q_2/s^3 \,.\, (1-v^2/c^2) \,.\, x \tag{4.2}$$

$$B_z = q_2/s^3 \,.\, (1-v^2/c^2) \,.\, v/c \,.\, y \tag{4.3}$$

$$B_y = 0. \tag{4.4}$$

Similarly comparing

$$F_y = q_1 \,.\, E_y + q_1/c \,.\, (u_z \,.\, B_x - u_x \,.\, B_z)$$

with eqn. (3.5)

$$F_y = q_1 \,.\, q_2/s^3 \,.\, (1-v^2/c^2) \,.\, \{1-(v \,.\, u_x)/c^2\} \,.\, y,$$

we conclude that at A in fig. 2

$$E_y = q_2/s^3 \,.\, (1-v^2/c^2) \,.\, y \tag{4.5}$$

$$B_x = 0 \tag{4.6}$$

and

$$B_z = q_2/s^3 \,.\, v/c \,.\, y \,.\, (1-v^2/c^2) \qquad \text{as before}$$

Comparing

$$F_z = q_1 \,.\, E_z + q_1/c \,.\, (u_x \,.\, B_y - u_y \,.\, B_x)$$

with the equation

$$F_z = 0$$

obtained by putting $z=0$ in eqn. (3.6) we conclude that

$$\begin{aligned} E_z &= 0 \\ B_y &= 0 \\ B_x &= 0. \end{aligned} \tag{4.7}$$

Thus only the terms E_x, E_y and B_z are not zero in the case shown in fig. 2. Thus $\mathbf{B}$ is perpendicular to $\mathbf{E}$. It is left as an exercise to the reader to show that $\mathbf{B}$ is perpendicular to $\mathbf{E}$ in the case when the z coordinate of the position of the test charge is not zero. It will be observed that all the terms in eqns. (3.4), (3.5) and (3.6) can be incorporated in the Lorentz force, confirming that eqn. (4.1) is adequate to define the force on the test charge q_1 (e.g. it is not necessary to introduce terms proportional to u^2, that is the square of the velocity of the test charge.) Hence the formula for the Lorentz force could be said to follow from eqns. (3.4), (3.5) and (3.6).

The components of the electric intensity are given by eqns. (4.2) and (4.5). The resultant electric intensity at A is in the XY plane at an angle β to the OX axis given by

$$\tan\,\beta = E_y/E_x = y/x.$$

Hence $\beta=\theta$ so that the electric field lines emanate radially from the ‘ present ’ position of the *uniformly* moving charge q_2. By ‘ present ’ position is meant the position of the charge q_2 in the inertial frame Σ at the time when the electric and magnetic field strengths due to q_2 are measured at the point A in Σ.

Hence

$$\mathbf{E} = q_2/s^3 \,.\, (1-v^2/c^2) \,.\, \mathbf{r}$$

where $\mathbf{r}$ is a vector of length OA pointing in the direction O to A.

Now from eqn. (3.2)

$$\begin{aligned} s^2 &= x^2 + (1-v^2/c^2) \,.\, y^2 \\ &= x^2 + y^2 - v^2/c^2 \,.\, y^2 \\ &= r^2 \,.\, (1 - v^2/c^2 \,.\, y^2/r^2) \\ &= r^2 \,.\, (1 - v^2/c^2 \,.\, \sin^2\theta) \end{aligned}$$

$$\therefore \quad \mathbf{E} = \frac{q_2 \cdot \mathbf{r}}{r^3} \cdot \frac{(1-v^2/c^2)}{(1-v^2/c^2 \,.\, \sin^2\theta)^{3/2}}. \tag{4.8}$$

Notice the field is radial, is proportional to $1/r^2$, and reduces to q_2/r^2 when $v=0$. However, except when $v=0$ the field is not isotropic. From eqn. (4.8) it can be seen that the electric field is reduced in the direction of motion ($\theta=0$) in the ratio $(1-v^2/c^2)$ and increased in the direction at right angles to the direction of motion ($\theta=\pi/2$) in the ratio $1/\sqrt{(1-v^2/c^2)}$ compared with the electrostatic field. In fig. 3 a typical case of the electric field produced by a charge q_2 is shown.

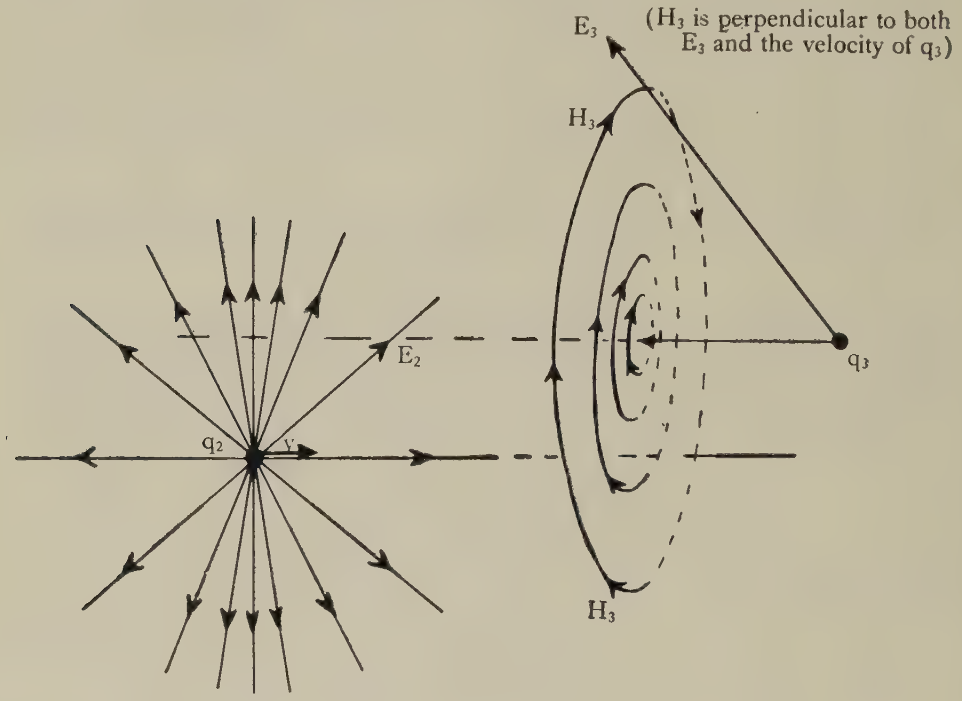
It can be shown by considering the case when z is not zero at the position of the test charge q_1 that the magnetic induction due to the uniformly moving charge q_2 is in general given by

$$\mathbf{B} = \frac{\mathbf{v}}{c} \times \mathbf{E}. \tag{4.9}$$

The proof for $z \neq 0$ is left as an exercise for the reader. In the case shown in fig. 2, $E_z=0$ and the only component of v is in the x direction, hence, according to eqn. (4.9) the only component of B should be

$$B_z = v_x/c \,.\, E_y = v/c \,.\, q_2/s^3 \,.\, (1-v^2/c^2) \,.\, y.$$

This is in agreement with eqns. (4.3), (4.4) and (4.6). The lines of magnetic induction are perpendicular to both the velocity of the charge q_2 and the lines of electric intensity which emanate radially from q_2. The lines of B therefore form closed circles in the plane perpendicular to the direction of motion of the charge producing the field and concentric with the direction of motion of the charge producing the field. The lines of magnetic induction in one plane are sketched in fig. 3 for a charge q_3 moving uniformly to the left.



(a) (b)

Fig. 3. In fig. 3 (*a*) the electric field produced by a charge q_2 moving with uniform velocity is shown. In fig. 3 (*b*) the magnetic field produced by the charge q_3 moving to the left with uniform velocity, is sketched in one plane only. Notice the magnetic field is perpendicular to both E_3 and the velocity of the charge q_3.

As the velocity of a charge tends to the velocity of light the lines of E bunch closer and closer to the direction perpendicular to the direction of motion, with B remaining perpendicular to E. Hence at very high velocities ($v \to c$) the electromagnetic field of a moving charge resembles the field in a plane wave. In the limit $v=c$ eqns. (4.8) and (4.9) show that $\mathbf{E}$ would be perpendicular to both $\mathbf{v}$ and $\mathbf{B}$, and in c.g.s. units $E=H$ in empty space.

We can rewrite eqn. (4.3) as

$$B_z=\frac{v}{c} \cdot \frac{q_2}{r^2} \cdot \frac{(1-v^2/c^2)}{(1-v^2/c^2 \sin^2\theta)^{3/2}} \cdot y/r.$$

When $v \ll c$, as is the case for the charges moving in normal conduction currents, then

$$B_z \longrightarrow v/c \,.\, q_2/r^2 \,.\, \sin\theta$$

where q_2 is in e.s.u. This is in agreement with the Biot-Savart Law for the magnetic fields due to current elements. It must be remembered that there is an electric force between the two charges in addition to the magnetic force.

Using eqns. (4.1) and (4.9) we have

$$\mathbf{F}=q_1\mathbf{E}+q_1/c\,.\,\mathbf{u}\times\mathbf{v}/c\times\mathbf{E}.$$

The second term is of order uv/c^2 and is of second order. Hence, provided both u and v are less than c, the magnetic force between the two charges is always less than the electric force. Substitution of the expression for $\mathbf{E}$ appropriate to the conditions shown in fig. 5 of Paper I yields the eqn. (5.1) of Paper I.

It must be stressed that according to the theory of special relativity there is no absolute frame of reference, all inertial frames being equally suitable. However, the numerical values of $\mathbf{E}$ and $\mathbf{B}$ depend on the inertial frame chosen, e.g. in Σ' of fig. 1 the magnetic induction due to q_2 would be zero whatever the velocity of the test charge as q_2 is at rest in Σ', whereas the values of $\mathbf{E}$ and $\mathbf{B}$ in Σ are given by eqns. (4.8) and (4.10).

In carrying out the calculations, in order to apply the transformations of the theory of special relativity, we assumed that the charge q_2 moves with uniform velocity. If we assume that the electric and magnetic fields are propagated with a finite velocity c, then the fields at A (in fig. 2) at a time $t=0$ (at which time q_2 is at the origin) must have originated at the charge q_2 at some previous point of its path, which we shall call its ' retarded ' position. Thus it is better in the case of non-uniform motion to express the fields in terms of the ' retarded ' position of the charge. It is possible to convert eqns. (4.8) and (4.10) into the Liénard-Wiechert potentials. This goes beyond the scope of these articles: the interested reader is referred to Smythe (1939). It is assumed in electromagnetic theory that the Liénard-Wiechert potentials depend only on the instantaneous velocity of the ' point ' charge at its ' retarded ' position, and are independent of its acceleration. They are assumed to be propagated with a velocity c in empty space independent of the subsequent motion of the charge. The Liénard-Wiechert potentials can also be derived from Maxwell's equations via the retarded potentials by allowing for the variation of the retarded time within the infinitesimal volume of the point charge (e.g. Panofsky and Phillips 1955).

5. DISCUSSION

Some text-books on special relativity imply that electromagnetism can be derived from Coulomb's law. This assumes the validity of the transformations of special relativity. It must be remembered that most of the so-called confirmations of these transformations are based on experiments of the Kaufmann-Bucherer type which use electron optics and assume the validity of classical electromagnetism in their interpretation (O'Rahilly 1938). If one wanted to base electromagnetism on the approach used in this paper it would be preferable to start the theory by postulating the transformations as we have done and basing the validity of the theory on whether or not its predictions agreed with experiments, rather than assuming that the transformations of special relativity can be based directly on ' induction ' from experiments. Naturally when teaching the subject for the first time electromagnetism must be introduced before relativity. In the author's view the most natural way of introducing special relativity is to show that the Lorentz transformations are the coordinate transformations which leave Maxwell's equations in the same form in all inertial frames. To a large extent electromagnetism and special relativity are the same theory. To quote Pauli, 1958 : " Since electron theory is in agreement with relativity, the latter cannot produce results which are not already contained in pre-relativistic Lorentz electron theory, as far as calculations of the electromagnetic field for given electronic motion are concerned."

However, few people realize how straightforward it is to go in the reverse direction from relativity to electromagnetism, without even introducing the transformations for **E** and **B**. The author feels that to see it done in this way brings out the essential unity inherent in classical electromagnetic theory. This unity is not so apparent when Maxwell's equations are developed on the basis of a number of diverse experimental phenomena plus the seemingly *ad hoc* assumption of the existence of displacement ' currents '.

On the approach adopted here, since the magnetic force follows from Coulomb's law, it is only necessary to introduce one constant into electrostatics and electromagnetism, for example it can be introduced at the outset in Coulomb's law, viz.

$$F = \mathrm{K} \,.\, \frac{q_1 \cdot q_2}{r^2}.$$

In the c.g.s. system, which we have used throughout, the unit charge is defined so as to make K equal to unity. As an illustration of how the formulae look in rationalized M.K.S. units we put K equal to $1/4\pi\epsilon_0$ and measure all quantities in the appropriate M.K.S. units. Equations (3.4), (3.5), and (3.6) become

$$\begin{aligned} F_x &= \frac{q_1 \cdot q_2}{4\pi\epsilon_0 \,.\, s^3} \cdot (1 - v^2/c^2)\{x + v/c^2(y \,.\, u_y + z \,.\, u_z)\} \\ F_y &= \frac{q_1 \cdot q_2}{4\pi\epsilon_0 \,.\, s^3} \cdot (1 - v^2/c^2)\{1 - (v \,.\, u_x)/c^2\} \,.\, y \qquad (5.1) \\ F_z &= \frac{q_1 \cdot q_2}{4\pi\epsilon_0 \,.\, s^3} \cdot (1 - v^2/c^2) \,.\, \{1 - (v \,.\, u_x)/c^2\} \,.\, z. \end{aligned}$$

In M.K.S. units the Lorentz force becomes

$$\mathbf{F} = q_1 \, \mathbf{E}_2 + q_1 \, \mathbf{u} \times \mathbf{B}_2. \qquad (5.2)$$

The charge is measured in coulombs in both electrostatics and electromagnetism. The definition of B becomes

$$B = (F_{\text{mag.}})_{\max} / q_1 \cdot u \qquad (5.3)$$

where q_1 is in coulombs.

Notice that the factor c, which has the dimensions of velocity, is not included either in this formula or in the Lorentz force.

The electric and magnetic fields of the charge q_2 moving with uniform velocity v, discussed in § 4, now become

$$\mathbf{E} = \frac{q_2}{4\pi\epsilon_0} \cdot \frac{\mathbf{r}}{r^3} \cdot \frac{(1 - v^2/c^2)}{(1 - v^2/c^2 \,.\, \sin^2\theta)^{3/2}} \qquad (5.4)$$

$$\mathbf{B} = \frac{\mathbf{v}}{c^2} \times \mathbf{E} = \frac{q_2}{4\pi\epsilon_0 \,.\, c^2} \cdot \frac{\mathbf{v} \times \mathbf{r}}{\mathbf{r}^3} \cdot \frac{(1 - v^2/c^2)}{(1 - v^2/c^2 \,.\, \sin^2\theta)^{3/2}} \qquad (5.5)$$

The latter formula reduces, in the limit $v \ll c$ to the Biot-Savart formula, which expressed in M.K.S. units is

$$\mathbf{B} = \frac{\mu_0}{4\pi} \cdot q \cdot \frac{\mathbf{v} \times \mathbf{r}}{r^3} \tag{5.6}$$

provided we put

$$\epsilon_0 c^2 = 1/\mu_0$$

or

$$\frac{1}{\mu_0 \cdot \epsilon_0} = c^2. \tag{5.7}$$

The relation (5.7) is usually introduced either by substituting the experimental value of ϵ_0 and the value $4\pi \, . \, 10^{-7}$ for μ_0 into the formula $1/\mu_0 \, . \, \epsilon_0$ and showing that it is numerically equal to c^2 within experimental error, or by showing that Maxwell's equations can have wave solutions having a velocity $1/\sqrt{(\mu_0 \cdot \epsilon_0)}$ in empty space ; these waves are then identified with light waves. We have seen that the relation (5.7) is implicit in the theory of special relativity.

If one wants to introduce dimensions for electrical quantities one need only introduce a new dimension for electric charge and ϵ_0, μ_0, $\mathbf{E}$, $\mathbf{B}$ etc. can be expressed in terms of it plus mass, length and time. This is the approach generally adopted in the M.K.S.A. system. If in the c.g.s. system we took $q_{\text{e.s.u.}}$ as our fundamental electrical dimension it follows from a comparison of our definition of $\mathbf{B}$ in terms of $q_{\text{e.s.u.}}$ namely

$$B = \frac{(F_{\text{mag.}})_{\text{max.}}}{\frac{q_{\text{e.s.u}}}{c} \cdot u} \tag{4.1b}$$

and the conventional definition of B in e.m.u. namely

$$B = (F_{\text{mag.}})_{\text{max.}} \Big/ q_{\text{e.m.u.}} \cdot u$$

that $q_{\text{e.m.u.}}$ has the dimensions of $q_{\text{e.s.u.}}/c$. Hence it is only natural to expect quantities like current to have different dimensions in e.s.u. and e.m.u. These different dimensions have no special metaphysical significance as some people would lead us to believe, but merely arise naturally from the way the quantities are defined. Whether or not this is the natural way of defining them is, of course, another story.

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APPENDIX

Derivation of the transformation formulae for force: Eqns. (2.7)

For convenience we summarize the formulae introduced in § 2. If x, y, z and t are the coordinates in an inertial frame Σ and x', y', z' and t' are the coordinates in an inertial frame Σ' moving along the positive OX axis with uniform velocity v relative to Σ then we have :

$$\left.\begin{aligned} x' &= \frac{x-vt}{\sqrt{(1-v^2/c^2)}} \\ y' &= y \\ z' &= z \\ t' &= \frac{t-(v\,.\,x)/c^2}{\sqrt{(1-v^2/c^2)}}. \end{aligned}\right\} \qquad (A.1)$$

The inverse transformations are obtained by replacing x by x' etc. and v by $-v$. For velocities we have :

$$\begin{aligned} u_x &= \frac{u_x'+v}{\{1+(v\,.\,u_x')/c^2\}} \\ u_y &= \frac{u_y'\sqrt{(1-v^2/c^2)}}{\{1+(v\,.\,u_x')/c^2\}} \\ u_z &= \frac{u_z'\,.\,\sqrt{(1-v^2/c^2)}}{\{1+(v\,.\,u_x')/c^2\}}. \end{aligned} \qquad (A.2)$$

Now $u^2 = u_x{}^2+u_y{}^2+u_z{}^2$ and $u'^2=u_x'^2+u_y'^2+u_z'^2$.

Using eqns. (A.2) we have :

$$\begin{aligned} u^2 &= \frac{(u_x'+v)^2+(u_y'^2+u_z'^2)(1-v^2/c^2)}{\{1+(u_x'\,.\,v)/c^2\}^2} \\ &= \frac{(u_x'+v)^2+(u'^2-u'_x{}^2)(1-v^2/c^2)}{\{1+(u_x'\,.\,v/c^2\}^2} \end{aligned} \qquad (A.3)$$

For convenience, let

$$K = \frac{1}{\sqrt{(1-v^2/c^2)}} \qquad (A.4)$$

$$K_1 = \frac{1}{\sqrt{(1-u^2/c^2)}} \qquad (A.5)$$

$$K_1' = \frac{1}{\sqrt{(1-u'^2/c^2)}} \qquad (A.6)$$

We will now prove the relation

$$K_1 = K\,.\,K_1'\,.\,\{1+(u_x'\,.\,v)/c^2\} \qquad (A.7)$$

We have

$$\begin{aligned} 1-u^2/c^2 &= 1-\left[\frac{(u_x'/c+v/c)^2+(u'^2/c^2-u_x'^2/c^2)(1-v^2/c^2)}{\{1+(u_x'\,.\,v)/c^2\}^2}\right] \\ &= \left[\frac{1+(2\,.\,u_x'\,.\,v)/c^2+(u_x'^2\,.\,v^2)/c^4-u_x'^2/c^2-(2u_x'\,.\,v)/c^2}{\{1+(u_x'\,.\,v)/c^2\}^2}\right] \\ &\quad+\left[\frac{-v^2/c^2-u'^2/c^2+u_x'^2/c^2+(u'^2v^2)/c^4-{}^1u_x'^2.v^2)/c^4}{\{1+(u_x'\,.\,v)/c^2\}^2}\right] \\ &= \left[\frac{1-v^2/c^2-u'^2/c^2+(u'^2\,.\,v^2)/c^4}{\{1+(u_x'\,.\,v)/c^2\}^2}\right] \end{aligned}$$

i.e.

$$1-u^2/c^2 = \frac{(1-v^2/c^2)(1-u'^2/c^2)}{\{1+(u_x'\,.\,v)/c^2\}^2}.$$

Equation A.7 follows by taking the square roots of the reciprocals of both sides.

We now proceed to calculate the formulae for the transformation of force.

In Σ the force acting on a particle is defined as

$$F_x = \frac{\mathrm{d}}{\mathrm{d}t}(m \,.\, u_x) = m_0 \,.\, \frac{\mathrm{d}}{\mathrm{d}t}(K_1 \,.\, u_x)$$

$$F_y = \frac{\mathrm{d}}{\mathrm{d}t}(m \,.\, u_y) = m_0 \,.\, \frac{\mathrm{d}}{\mathrm{d}t}(K_1 \,.\, u_y)$$

$$F_z = \frac{\mathrm{d}}{\mathrm{d}t}(m \,.\, u_z) = m_0 \,.\, \frac{\mathrm{d}}{\mathrm{d}t}(K_1 \,.\, u_z)$$

where $m = m_0 \,.\, K_1$, m_0 being the rest mass.

Similarly in Σ'

$$F_x' = \frac{\mathrm{d}}{\mathrm{d}t'}(m' \,.\, u_x') = m_0 \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}(K_1' \,.\, u_x') \text{ etc.} \tag{A.8}$$

From equation A.1 we have differentiating with respect to t'

$$\frac{\mathrm{d}t}{\mathrm{d}t'} = \frac{\mathrm{d}}{\mathrm{d}t'}\left\{\frac{t' + vx'/c^2}{\sqrt{(1 - v^2/c^2)}}\right\}$$
$$= K\,\{1 + (u_x' \,.\, v)/c^2\}$$

Hence

$$F_x = m_0 \,.\, \frac{\mathrm{d}t'}{\mathrm{d}t} \,.\, \frac{\mathrm{d}(K_1 \,.\, u_x)}{\mathrm{d}t'} = m_0 \,.\, \frac{1}{K\{1 + (u_x' \,.\, v)/c^2\}} \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}(K_1 \,.\, u_x)$$

Substituting for u_x from eqns. (A.2) and for K_1 from eqn. (A.7) we have

$$F_x = \frac{m_0}{K\{1 + (u_x' \,.\, v)/c^2\}} \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}\left[K \,.\, K_1' \,.\, \{1 + (u_x' \,.\, v)/c^2\}\frac{(u_x' + v)}{\{1 + (u_x' \,.\, v)/c^2\}}\right]$$

$$= \frac{m_0}{\{1 + (u_x' \,.\, v)/c^2\}} \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}\{K_1'(u_x' + v)\}$$

$$= \frac{m_0}{\{1 + (u_x' \,.\, v)/c^2\}} \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}(K_1' \,.\, u_x') + \frac{m_0 \,.\, v}{\{1 + (u_x' \,.\, v)/c^2\}} \,.\, \frac{\mathrm{d}K_1'}{\mathrm{d}t'}.$$

It must be remembered that Σ' moves with uniform velocity v relative to Σ so that v is independent of t'.

Add and subtract

$$\frac{m_0 \,.\, u_x' \,.\, v/c^2}{\{1 + (u_x' \,.\, v)/c^2\}} \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}(K_1' \,.\, u_x')$$

to the right hand side as follows :

$$F_x = \frac{m_0\{1 + (u_x' \,.\, v)/c^2\} \cdot \frac{\mathrm{d}}{\mathrm{d}t'}(K_1' \,.\, u_x')}{\{1 + (u_x' \,.\, v)/c^2\}} + \frac{m_0 v}{\{1 + (u_x' . v)/c^2\}} \cdot \left[-\frac{u_x'}{c^2} \,.\, \frac{\mathrm{d}}{\mathrm{d}t'}(K_1' u_x') + \frac{\mathrm{d}K_1'}{\mathrm{d}t'}\right]$$

$$F_x = F_x' + \frac{m_0 \,.\, v}{\{1 + (u_x' \,.\, v)/c^2\}} \cdot \left[(1 - u_x'^2)/c^2 \frac{\mathrm{d}}{\mathrm{d}t'}(K_1') - \frac{K_1' \,.\, u_x'}{c^2} \,.\, \frac{\mathrm{d}u_x'}{\mathrm{d}t'}\right] \tag{A.9}$$

Now

$$(1 - u_x'^2/c^2) = 1 - \frac{(u'^2 - u_y'^2 - u_z'^2)}{c^2}$$

that is

$$(1 - u_x'^2)/c^2 = u_y'^2/c^2 + u_z'^2/c^2 + 1/K_1'^2 \tag{A.10}$$

Differentiating with respect to t' we have

$$-\frac{2u_x'}{c^2} \,.\, \frac{\mathrm{d}u_x'}{\mathrm{d}t'} = \frac{2u_y'}{c^2} \,.\, \frac{\mathrm{d}u_y'}{\mathrm{d}t'} + \frac{2u_z'}{c^2} \,.\, \frac{\mathrm{d}u_z'}{\mathrm{d}t'} - \frac{2}{K_1'^3} \,.\, \frac{\mathrm{d}K_1'}{\mathrm{d}t'}.$$

Hence

$$-\frac{K_1' \,.\, u_x'}{c^2} \,.\, \frac{\mathrm{d}u_x'}{\mathrm{d}t'} = \frac{K_1' \cdot u_y'}{c^2} \,.\, \frac{\mathrm{d}u'_y}{\mathrm{d}t'} + \frac{K_1' . u_z'}{c^2} \cdot \frac{\mathrm{d}u_z'}{\mathrm{d}t'} - \frac{1}{K_1'^2} \,.\, \frac{\mathrm{d}K_1'}{\mathrm{d}t'} \tag{A.11}$$

Substituting from eqns. (A.10) and (A.11) we have

$$F_x = F_x' + \frac{m_0 v}{\{1+(u_x' \, . \, v)/c^2\}} \left\{ \left(\frac{u_y'^2}{c^2} + \frac{u_z'^2}{c^2} + \frac{1}{K_1'^2} \right) \frac{\mathrm{d}K_1'}{\mathrm{d}t'} + \frac{K_1' \, . \, u_y'}{c^2} \, . \, \frac{\mathrm{d}u_y'}{\mathrm{d}t'} + \right.$$

$$\left. + \frac{K_1' \, . \, u_z'}{c^2} \, . \, \frac{\mathrm{d}u_z'}{\mathrm{d}t'} - \frac{1}{K_1'^2} \, . \, \frac{\mathrm{d}K_1'}{\mathrm{d}t'} \right\}$$

$$= F_x' + \frac{m_0 \, . \, v}{c^2\{1+(u_x' \, . \, v)/c^2\}} \left\{ u_y' \, . \, \frac{\mathrm{d}}{\mathrm{d}t'} (u_y' \, . \, K_1') + u_z' \, . \, \frac{\mathrm{d}}{\mathrm{d}t'} (u_z' \, . \, K_1') \right\}$$

Hence

$$F_x = F_x' + \frac{u_y' \, . \, v}{(c^2 + u_x' \, . \, v)} \, . \, F_y' + \frac{u_z' \, . \, v}{(c^2 + u_x' \, . \, v)} \, . \, F_z'.$$

which is the first of eqns. (2.7)

$$F_y = m_0 \, . \, \frac{\mathrm{d}}{\mathrm{d}t} (K_1 \, . \, u_y)$$

$$= \frac{m_0}{K\{1+(u_x' \, . \, v)/c^2\}} \, . \, \frac{\mathrm{d}}{\mathrm{d}t'} (K_1 \, . \, u_y)$$

Now

$$K_1 = K \, K_1' \{1+(u_x' \, . \, v)/c^2\} \qquad \text{(A.7)}$$

and

$$u_y = \frac{u_y'}{K\{1+(u_x' \, . \, v)/c^2\}} \qquad \text{(A.2)}$$

$$\therefore \quad K_1 \, . \, u_y = K_1' \, . \, u_y'$$

$$F_y = \frac{m_0}{K\{1+(u_x' \, . \, v)/c^2\}} \, . \, \frac{\mathrm{d}}{\mathrm{d}t'} (K_1' \, . \, u_y' \, .$$

$$\therefore \quad F_y = \frac{c^2 \sqrt{(1 - v^2/c^2)}}{c^2 + u_x' \, . \, v} \, . \, F_y'$$

Similarly

$$F_z = \frac{c^2 \sqrt{(1 - v^2/c^2)}}{c^2 + u_x' \, . \, v} \, . \, F_z'.$$

These two equations complete the transformation formulae (2.7) for force.

Interference Filters

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SUMMARY

An account is given of the application of thin-film interference to the production of both anti-reflection and high-reflection coatings on glass, and the more recent developments of multiple-film, Fabry–Perot, and frustrated total internal reflection filters. Applications include the ' cold mirror ' which reflects visible light and transmits infra-red, and the ' dichroic mirror ' used for colour separation in the colour television camera which is described briefly.

1. ELEMENTARY THEORY

The simplest theory of thin-film interference concerns itself only with path-differences and the patterns observed when these vary. The simple school experiments, for example ' Newton's rings ', usually manage to contrive the superposition of only two beams of comparable amplitude without worrying why. Here we shall be dealing with thin films of uniform thickness, so that there is no pattern; but of course the conditions for destructive interference (path difference $\lambda/2$, and phase difference of π, or odd multiples of these) and for reinforcement still apply. It is necessary to consider the reflection coefficient (or reflectance) and the transmission coefficient at the boundaries of the film, and the electromagnetic theory of light enables these to be calculated. The results, which are derived in the standard text books, are quoted below. Apart from this one feature, the whole of the theory will be presented in the very simplest terms.

The reflection coefficient R of a dielectric surface for light striking at normal incidence is given by:

$$R=(\mu-1)^2/(\mu+1)^2 \tag{1}$$

where μ is the refractive index of the dielectric. Thus for a glass having $\mu=1{\cdot}6$ the reflection coefficient is 0·053 so that at normal incidence about 5 per cent of the light is reflected.

The reflection coefficient for a metallic surface for normal incidence is given by:

$$R=\frac{(\mu-1)^2+k^2}{(\mu+1)^2+k^2}. \tag{2}$$

In the derivation of this expression the refractive index μ in the formula previously quoted is replaced by a complex refractive index $(\mu-jk)$. Values of μ and k (as for equation (2)) obtained for silver indicate a reflection coefficient of 95 per cent at normal incidence.

When light is incident at the surface of an optically less dense medium at an angle greater than the critical angle, the intensity of the reflected wave is equal to that of the incident wave. There is however a wave propagated into the less dense medium which travels along the boundary surface within a layer which is about 2–3 wavelengths thick. If the less dense medium at which critical reflection occurs is in the form of a very thin film, then penetration of the film by the transmitted wave takes place and control of the film thickness permits accurate control of small amounts of transmitted light. This property is referred to later when dealing with frustrated total internal reflection filters.

In the simple theory of thin-film interference, the reflection coefficient at normal incidence is usually small and only one or two reflections are considered for the transmitted beam. Extinction of the reflected light occurs when the path difference is a whole number of wavelengths, i.e. when:

$$p\lambda = 2\mu d \cos r \tag{3}$$

where r is the angle of refraction, d the film thickness and p is an integer. The fact that there is cancellation and not reinforcement is due to the phase change of π which takes place on reflection at the surface of the denser medium; and the same equation gives the condition also for maximum intensity of the *transmitted* beam. When surface reflection is higher, as it is for films of high refractive index, or when metallic reflecting surfaces form the two sides of the thin dielectric film, the simple theory is inadequate. Multiple reflection within the film occurs (fig. 1), and then, as shown in the standard advanced textbooks on light, the fraction T of light energy transmitted (*if there is no absorption*) is given by:

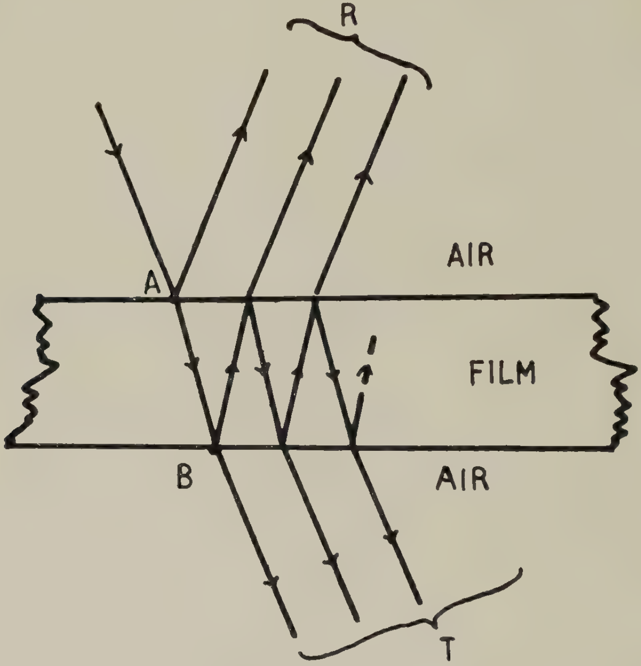

Fig. 1. Multiple reflections contributing to the reflected beam R and the transmitted beam T for a thin film.

$$T = \frac{1}{1 + \frac{4R}{(1-R)^2} \sin^2 \frac{1}{2}\phi} \tag{4}$$

where R is the reflection factor of the front and rear film surfaces, assumed the same for both, and ϕ is the phase angle retardation produced by the single passage of the wave through the thickness of the film. As ϕ depends on the wavelength, for a given film thickness, and a parallel beam of light at normal incidence, the transmission factor of the film as a function of wavelength can be determined. Transmission is a minimum when $\sin \frac{1}{2}\phi$ is unity and a maximum when $\sin \frac{1}{2}\phi$ is zero. If however R is small this variation of transmitted intensity with wavelength will be very small. When R is increased by silvering the two film surfaces the situation becomes markedly different. Suppose $R=0{\cdot}8$; then

$$T=\frac{1}{1+80\sin^2 \frac{1}{2}\phi}. \tag{5}$$

In this case, when $\sin \frac{1}{2}\phi=1$ the transmission is very small indeed and when $\sin \frac{1}{2}\phi=0$ the transmission is approximately 100 per cent. Absorption of light energy in silver films brings the transmission of such filters much below 100 per cent. By using reflecting coatings of higher efficiency, transmissions of up to 70 per cent can be achieved. Thus at a wavelength for which $\sin \frac{1}{2}\phi$ is zero the transmission of light through such a film is high and yet the light has passed through two reflecting surfaces each with a reflection coefficient of 0·8!

2. Blooming

It had been observed as early as 1892 that a tarnished photographic lens transmitted more light than a new one; but it was not then appreciated that this was an interference effect, in which the thickness of the lens coating and its refractive index both play important roles. When glass is coated with a thin transparent film, some light is reflected at the front face of the film and some at the film-glass interface. For normal incidence and a coating of refractive index $\mu=\sqrt{(\mu_{\text{glass}})}$, these two reflected waves have equal amplitudes. Further, both reflections occur at the surfaces of denser media. The simple ‘two beam’ theory can here be applied.

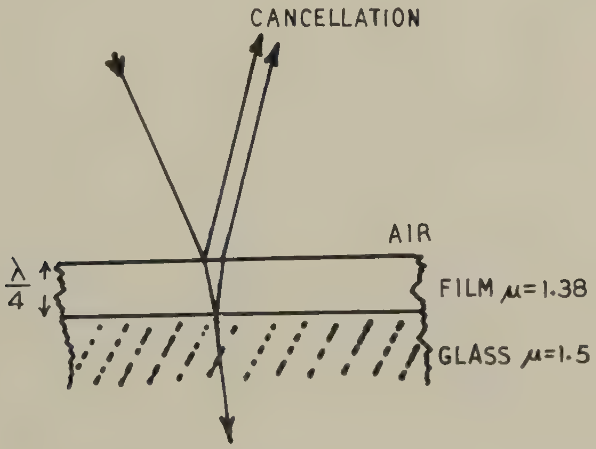

Fig. 2. An anti-reflection coating of magnesium fluoride on glass.

If the film thickness is one quarter of a wavelength for light in the middle of the visible spectrum (say 5500 Å) then the phase difference introduced between the interfering beams will be π. As the amplitudes of the two beams are equal, complete cancellation takes place and no light at this wavelength is reflected at the film surface (fig. 2). For other wavelengths the phase relationship of the reflected beams is not that for complete cancellation and some red and blue light is reflected; it is for this reason that bloomed lenses usually look purple. A lens could equally well be coated to eliminate reflection at any chosen wavelength. Figure 3 illustrates the effect of such a coating.

As glasses used for photographic lenses usually have refractive indices in the range 1·5 to 1·8, a coating material with a refractive index of from 1·23 to 1·34 is required. The fluorides of sodium and calcium have refractive indices in this range but are not sufficiently durable. Magnesium fluoride has a refractive index of 1·38 and although this lies outside the ideal range the film is robust. In manufacture, the lens surface is first washed and then cleaned by electrical discharge *in vacuo* immediately before the vacuum evaporation of the fluoride which condenses on the glass surface. The colour of a test surface, which is observed as deposition progresses, indicates the film thickness, and the process is stopped when this has reached the necessary quarter-wavelength.

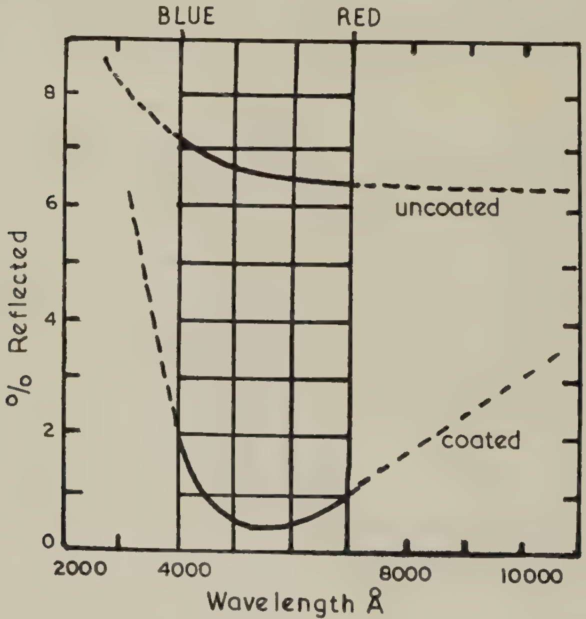

Fig. 3. Reflectance curves for coated and uncoated glass.

Such surface coatings are amazingly durable and adhere strongly to the glass. A typical test on a single lens from a batch is to stroke the surface with a rubber impregnated with emery powder and loaded to exert a pressure of 112 lb wt. per sq. in. One hundred double strokes should be required to change the coating colour from purple to straw.

3. Mirrors

Interference increases the light reflected by a glass surface coated with a quarter-wave thickness of material of greater refractive index. Such a substance is titanium dioxide, refractive index of 2·7 (fig. 4). Here, whereas a phase change of π is introduced by the first reflection at the surface of the denser film, the second reflection at the film-glass interface takes place at the surface of a

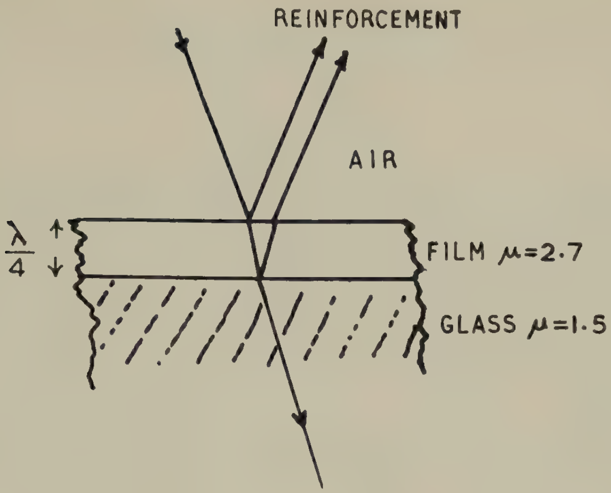

Fig. 4. Increased reflectance using a film of titanium dioxide.

less dense medium and there is, therefore, no change of phase. Thus by path difference we have one π phase change, and by reflection a further π, so that the emerging beams reinforce one another. The film is a quarter wavelength thick for only one wavelength of course and the reflectance is not so strong at wavelengths different from this chosen one. Nevertheless, a graph of reflectance against wavelength for such a coating is remarkably flat and is seen in fig. 5 where a glass surface has been given a 45 per cent reflectance and 55 per cent

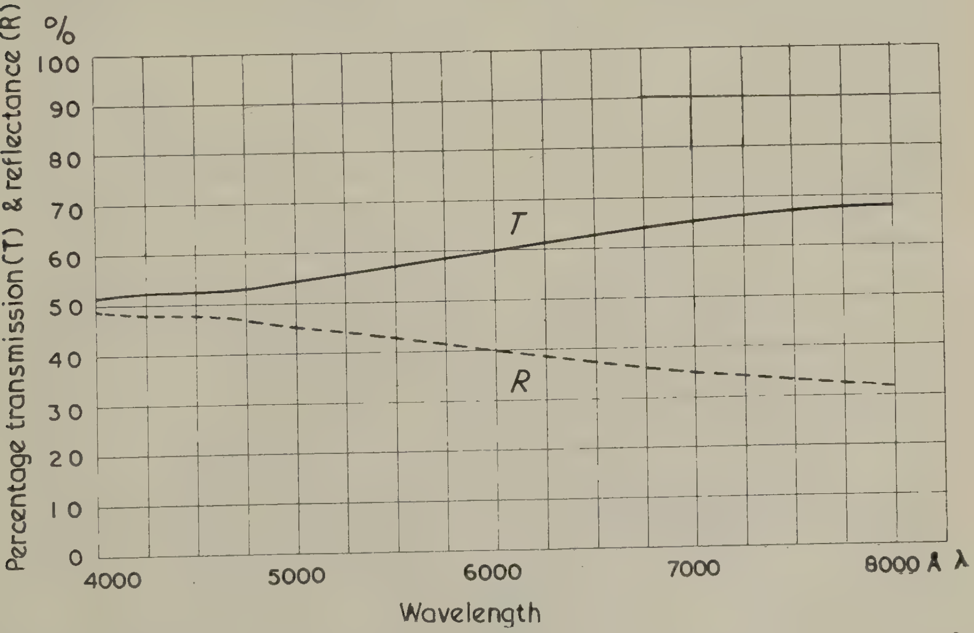

Fig. 5. Reflectance and transmission curves for an achromatic beam splitter, at 45° incidence.

transmission by coating the surface with titanium dioxide. It is to all intents and purposes achromatic. For a high refractive index film of this type, multiple reflection within the film leads to a reflection coefficient

$$R=\frac{(\mu_2{}^2-\mu_1\mu_3)^2}{(\mu_2{}^2+\mu_1\mu_3)^2} \tag{6}$$

where μ_1, μ_2 and μ_3 are the refractive indices of air, film and glass support (Messner 1943, 1947). So, when $\mu_2=2{\cdot}7$ and $\mu_1=1{\cdot}5$, $R\simeq0{\cdot}45$. An advantage of such a semi-transparent mirror is its very low light loss compared with the semi-silvered mirrors previously employed.

The titanium dioxide film is formed by first evaporating titanium metal on to the glass supporting surface which is rotated in a vacuum. During this process the film thickness is monitored by means of a light beam of a chosen ‘ control wavelength ’ and photocell. When the light transmission of the metal film falls to about 5 per cent the process is stopped and the mirror carefully heated to a temperature of about 400°C for two hours in air, to oxidize the film completely. Finally, an anti-reflection coating is applied to the rear side of the mirror. Semi-transparent mirrors of this type have many applications as beam splitters.

4. Multilayer films (dichroic mirrors)

In 1940, experiments with multilayer films were begun in an attempt to reproduce the effects of selective colour reflection and transmission exhibited by certain crystals. Lord Rayleigh (1888) had observed these effects and he wrote:

> “ In order to explain the vigour and purity of the colour reflected in certain crystals it is necessary to suppose that there are a considerable number of thin surfaces disposed at approximately equal intervals. At each angle of incidence there would be a particular wavelength for which the phases of the several reflections are in agreement. The selection of light of a particular wavelength would thus take place upon the same principle as in diffraction spectra and might reach a high degree of perfection ”.

Crystals of potassium chlorate can be made to have this property and a recipe for their preparation is given in R. W. Wood’s *Physical Optics*.

Multilayer films are laid down by vacuum evaporation, and usually consist (fig. 6) of alternate high and low refractive index materials; magnesium fluoride

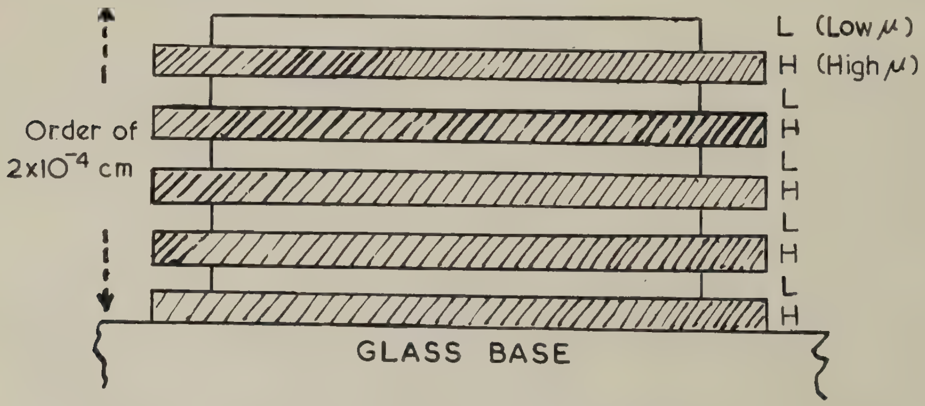

Fig. 6. Diagram of a multilayer filter.

is the low refractive index material ($\mu = 1{\cdot}38$) and zinc sulphide is the high refractive index material ($\mu = 2{\cdot}3$). If all the layers are one quarter wavelength thick then fairly wide band reflectance occurs but the reflectance varies more and more with wavelength as the thickness of the layers and the number of the layers increase. As so many variables are concerned in the deposition of a number of layers to achieve particular characteristics, there is not at present any synthetic method for designing a multilayer film to have specified properties. The trial and error methods practised start with a multilayer film known to give characteristics approaching those of the required mirror; then (using a digital computer), film thicknesses can be adjusted to correct for the small differences between the initial and the desired characteristic. Thicknesses of individual layers vary from $\frac{1}{8}$ wavelength to 1 wavelength. From 5 to 35 layers may be employed.

Such multilayer films may pass very narrow spectral bands and have greater efficiency and permanency than normal pigmented colour filters. There is little absorption, so that light which is not transmitted is reflected, whence transmitted and reflected light are complementary colours. Figure 7 shows some typical transmission/wavelength curves.

When narrow transmission bands are required, auxiliary filters are used to suppress the wide transmission bands which occur on either side of the required band. Coloured glass with suitable transmission characteristics is normally cemented to the interference filter to eliminate the short-wave sideband. The

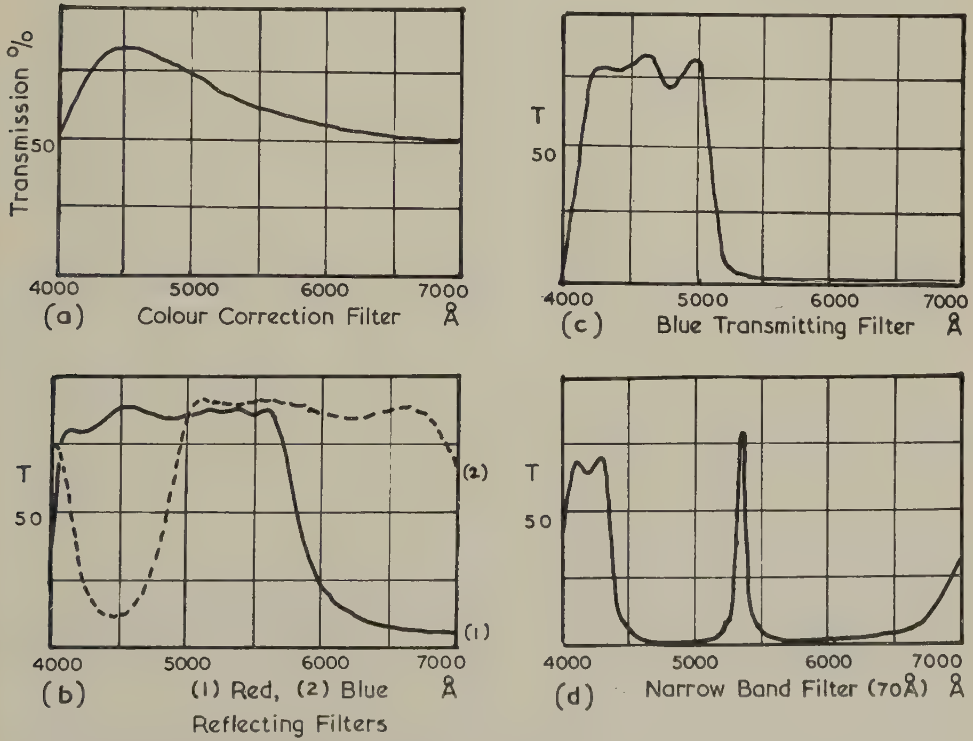

Fig. 7. Transmission characteristics of multilayer filters. (*a*) Colour-correction filter. (*b*) Full line, red reflector; broken line, blue reflector. (*c*) Blue transmitting filter. (*d*) Narrow band filter.

long-wave sideband is usually only partially suppressed, since suitable colour glasses are rare. The auxiliary filter somewhat reduces the desired peak of the interference filter transmission. This peak can be placed at any point in the spectrum from 4 200 Å to 15 000 Å, i.e. from the shortest wavelengths of the visible spectrum to well into the infra-red. In the ranges 4 200 Å to 5 000 Å, filters can be made with 80 per cent transmission and 70 Å bandwidth, or 60 per cent transmission and 30 Å bandwidth (fig. 7*d*).

5. Applications

5.1. *The cold mirror*

The cold mirror is a multilayer filter designed to reflect visible light strongly, and to transmit the infra-red (fig. 8). The low absorption of energy by the filter means that the mirror remains cool even in the most powerful illuminating systems; and the high melting-point coatings are more durable than glass. Experiments using cold mirrors in conjunction with 50 000-lumen arc lamps seem to promise success for this application.

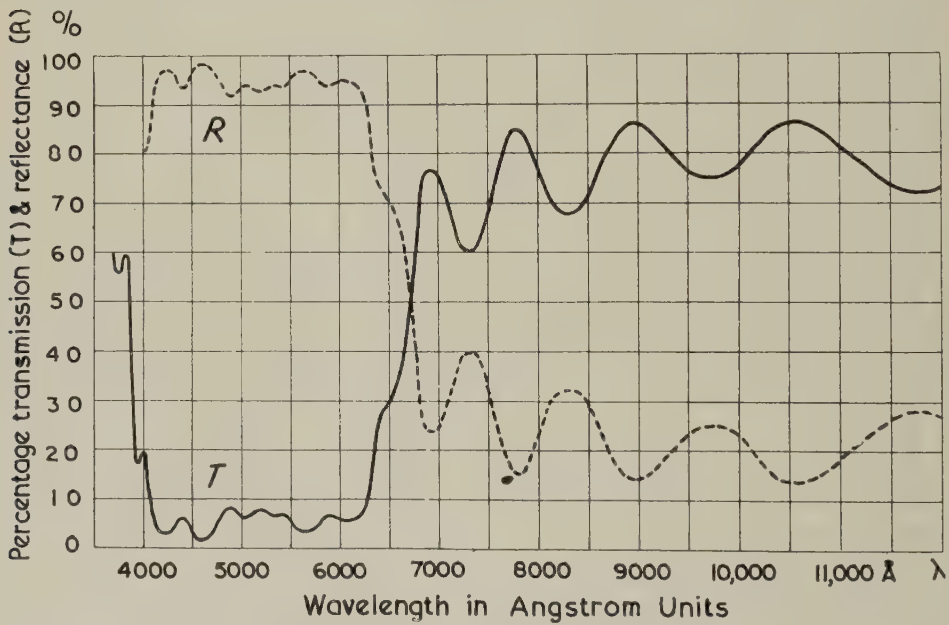

Fig. 8. Reflectance and transmission curves for a 'cold mirror', at 45° incidence.

5.2. *Colour television*

The problem here is to produce three images in red, green and blue from the light entering the turret lens of the colour television camera, without the light losses incurred by beam splitters and ordinary colour filters. Two multilayer filters (as fig. 7*b*) separate the light from the turret lens into its 'three colour' components (fig. 9). Two further surface silvered mirrors are used so that the television tubes in the camera may be more conveniently mounted. A pair of dichroic mirrors introduced between a lens and its principal focus can raise problems of optical design, especially when the focal length of the turret lens is short, for instance 2 or 3 in.

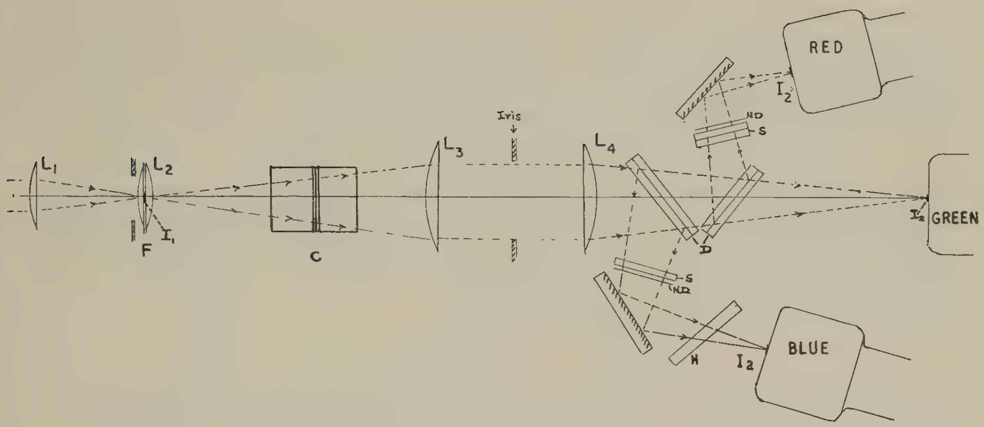

Fig. 9. The optical system of the colour television camera.

By means of two ' relay ' lenses L_3 and L_4, the first image I_1 produced by the turret lens L_1 is reproduced at I_2. The light between L_3 and L_4 is parallel and this is a convenient place for the iris which forms the overall gain control of the system. The space between L_4 and I_2 accommodates the dichroic mirrors, D, whilst the space between I_1 and L_3 contains two optical flats, C, which are equal in the thickness to the slabs of the dichroic mirrors but are mounted in planes perpendicular to the mirrors themselves, in order to correct the astigmatism which is introduced by the inclined slabs of glass. They function here by predistorting the optical system, cancelling in advance the distortion given by the dichroic mirrors. Unfortunately the predistortion is too great for the blue component of the beam as this is reflected at the first dichroic and passes through glass equivalent to the thickness of a single slab. (The multilayer film is sandwiched between two glass slabs of equal thickness so that reflection at the film means a double passage through half the total mirror thickness). A further slab of optically flat glass, H, must therefore be introduced in the blue beam to allow full cancellation of the predistortion. The red and blue components both travel through glass which is equal in thickness to twice the thickness of a single mirror, and no further compensation is necessary. The field lens, L_2, deviates the light forming the edges of the image at I_1 through the first relay lens; the field lens has no effect on the final image size.

The spectral characteristics of the available dichroics are not precisely those demanded by accurate colour analysis. What is required of them can only be calculated when the colours of the primary phosphors in the colour receiver are known. Ordinary colour glasses are used as shaping filters, S, to adjust the responses of the dichroic mirrors to what the receiving end demands. This adjustment is very slight and results in little light loss. Finally to balance the relative intensities of the three coloured images, neutral density filters, N.D., are used in two of the beams. These are adjusted in conjunction with the television camera tubes being used so that the overall sensitivities of the three channels are properly balanced. The turret lens L_1 is moved for focusing in just the same way as an ordinary camera lens. Dichroic mirrors formed by multilayer films on very thin glass sheets are now being produced; their astigmatism is negligible.

Colour synthesis at the colour television receiver is achieved with three primaries which are the colours of the three fluorescing phosphors. The brightnesses of the phosphor colours are determined by the voltage outputs from the three camera tubes. These voltages can be adjusted experimentally so that the colour reproduced by the colour television receiver matches in colour and brightness each wavelength in turn of an equal energy spectrum (as far as this is possible without introducing concepts of negative colour). As a preliminary, it is arranged that when the three voltages are equal the receiver reproduces white. A plot of voltage against wavelength gives three curves which readers will recognize as being equivalent to plots of distribution coefficients of colorimetry. These curves must be the effective transmission characteristics of the dichroic mirrors, when the effects of the shaping filters, the camera photoelectric response, and the neutral density filters are superposed.

A study of the properties of the eye can result in a considerable reduction in the actual colour information which need be transmitted. For very fine picture detail no colour information is required. For medium fine detail, corresponding to frequencies of from about 0·5 to 1·5 megacycles per sec approximately in the British television system, two colour information only is given, the effective primaries being orange and cyan. The conversion of red-green-blue analysis into orange-cyan is achieved electrically by matrix circuits. Recent reports of the work of Dr. Land (1959) indicate that the eye can introduce colour into regions where no colour information appears to be supplied. By the use of red and white primaries he claims to have achieved satisfactory colour reproduction. Similar results have been achieved in colour television when the blue signal has been switched off. But for *accurate* colour reproduction in the larger areas of saturated colour three primaries are a minimum requirement.

6. METAL INTERFERENCE FILTERS

The metal interference filter (fig. 10) consists of a thin film of dielectric such as zinc sulphide or magnesium fluoride which may be from a quarter wavelength to several wavelengths thick, coated on each side by a semi-transparent reflecting layer either of aluminium, silver or (as will be seen later) by a multilayer film acting as a semitransparent reflector.

Fabry and Perot used this principle in their interferometer for determining the standard metre in terms of the wavelength of cadmium light. Their filters (' etalons ') were semi-silvered glass surfaces separated by quartz blocks; the dielectric film was air. Their smallest etalon had an air gap of about half a millimetre, vastly thicker than the dielectric film in a ' Fabry–Perot ' metal interference filter.

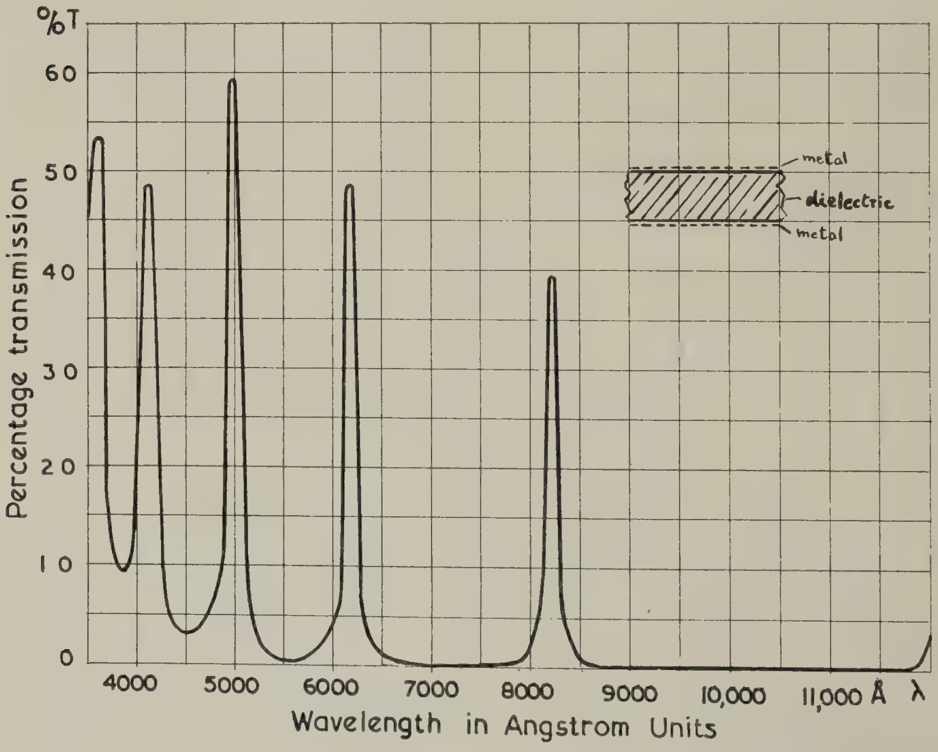

Fig. 10. Structure and response of a typical Fabry–Perot filter.

The expression for the transmitted light given earlier shows that when the film is half a wavelength thick at the control wavelength (5500 Å in this case) the spectral response is wide. As the layer is made thicker, first λ then $3\lambda/2$, the response becomes narrower and narrower but other peaks begin to appear in the visible spectrum (fig. 11).

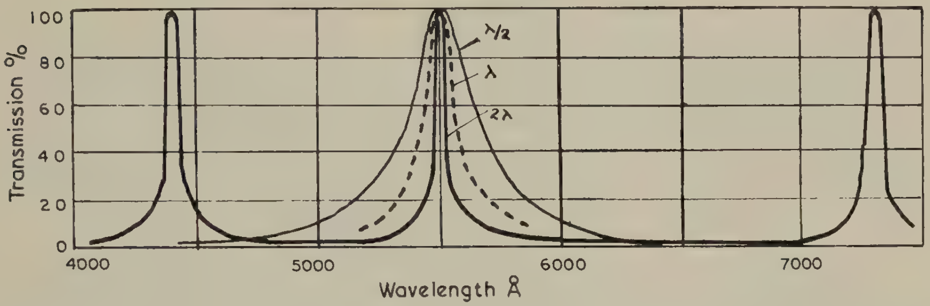

Fig. 11. Variation of the characteristic of Fabry–Perot filters with spacing film thickness; the lighter full line shows the broad peak for $\lambda/2$; the broken line the narrower peak for λ; and the thick full line the multiple narrow peaks for $3\lambda/2$

When a Fabry–Perot filter is tilted so that incidence is no longer normal the transmission band shifts towards shorter wavelengths and splits into two bands, polarised parallel to and perpendicular to the plane of incidence. Nevertheless it is possible to ' tune ' a Fabry–Perot filter by as much as 20 Å by tilting. In divergent or convergent beams the spectral transmission band becomes broader but this effect is minimised by using a dielectric spacer of high refractive index such as zinc sulphide, rather than one of low refractive index like magnesium fluoride.

Transmission bands of 150 Å are possible with this type of filter, with a peak transmission of 25 per cent. As compared with the multilayer dielectric type of filter they are less efficient, though unwanted transmission bands are easier to remove. This is done by combining two Fabry–Perot filters in series so that each transmits the desired wavelength but employs different zinc sulphide thicknesses. Thus only at the desired wavelength do both filters transmit.

6.1. *Applications*

Fabry–Perot filters have similar applications to the multilayer dielectric films. An interesting type of Fabry–Perot filter produced by Messrs. Barr & Stroud is called a Graded Spectrum Filter. This may consist of a circle of glass, with a central hole, on which is deposited a Fabry–Perot filter in the form of an annular strip with the zinc sulphide dielectric varying in thickness as the annular strip is traversed circumferentially, so the transmission wavelength varies as the wheel of glass is rotated. Using an incident collimated beam of light about 1 mm wide a transmission band of 300 Å is obtained which is smoothly variable throughout the visible spectrum. This graded filter is also available in strip form. As it is also possible to obtain circular graded neutral density filters of continuously variable density, a combination of variable wavelength light mixed with variable amplitude white could form the basis of an interesting colorimeter to measure dominant wavelength and saturation.

6.2. *Other Fabry–Perot filters*

Transmission may be improved by using multilayer dielectric films as the reflecting surfaces on either side of the spacer layer in place of the metal films.

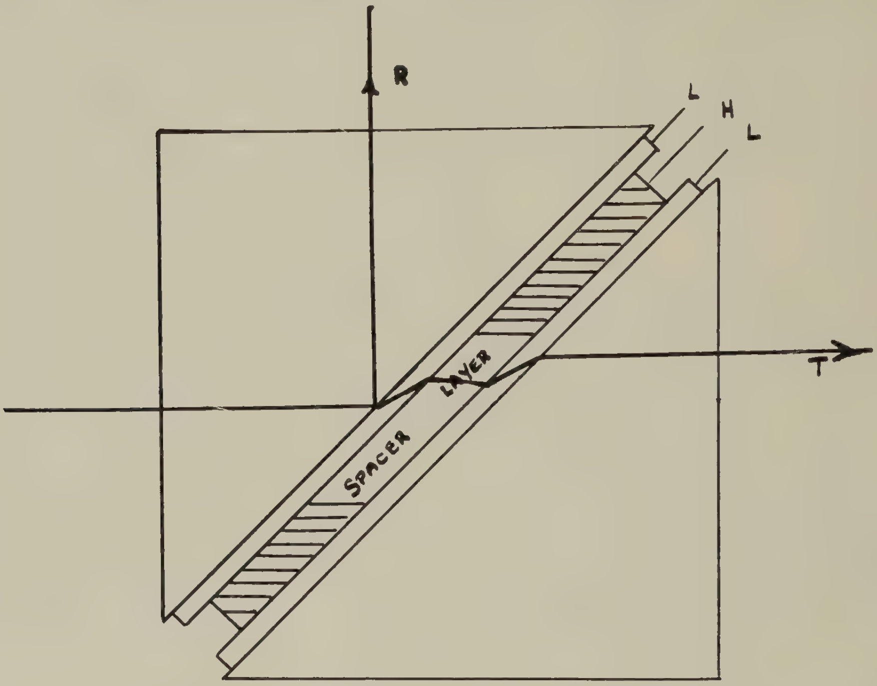

Fig. 12. Frustrated total internal reflection filter. The transmitted beam T is light which would be totally internally reflected as R *if the film* L *were* thick enough.

Another method uses critical reflection. A Fabry–Perot filter based on this method is called a frustrated total reflection filter (fig. 12). For, when a ray strikes an interface between one medium (glass) and a thin low refractive index film at an angle greater than the critical angle it is not totally internally reflected unless the thickness of the low index film exceeds a certain value and by controlling the thickness of this low refractive index coating a high reflection-to-transmission layer can be obtained without any light loss by absorption. A very carefully collimated beam is necessary if narrow transmission bands are required as this filter is very sensitive to angle of incidence. Its effectiveness extends well into the ultra-violet and infra-red, where Fabry–Perot metal interference filters are inefficient.

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Who will teach ?

by K. LAYBOURN
Chief Inspector of Schools, Bristol

> Tens of thousands of schoolboys have no idea in their heads but that they too will split the atom or ride on a rocket round the moon. Ask the average science student what he wants to do and he will say ' Research ', in the same tone one fancies as in the twelfth century he might have said ' Crusade '. As he sees it, science is an interesting and honourable profession, to be contrasted with what he believes to be the drabness of school teaching and municipal engineering . . .

This is a quotation from a recent Third Programme talk by John Ziman on what it takes to make a scientist. The conjunction of ' school teaching ' and ' municipal engineering ' is a significant one, reflecting the commonly held opinion that the job of the schoolmaster is a mechanical business, concerned more with discipline than with design, though the implied slight on municipal engineering would be equally gratuitous. For this situation the schoolmaster is himself much to blame. The fact that his calling has not yet been established as a profession cannot be divorced from his neglect of standards in regard to so many of his pupils. On the one hand, we have neglect in respect of training and qualification to teach; on the other, a gross persistence, at least so far as Science is concerned, in methods of teaching that are known to be ineffective with a majority of boys and girls.

In secondary schools of all kinds increasing difficulty is experienced in filling Science vacancies. Grammar schools no longer recruit First and Second Class Honours graduates as of right; indeed they often remain for longer or shorter periods without teachers for particular posts and must either employ non-graduates or increase the burden upon other members of staff. Experienced heads of departments are not being replaced, and this is probably the worst feature of a sombre situation. Virtually no women graduates are offering themselves for training to teach physics or chemistry in girls' grammar schools and many such schools are unable to provide courses in these subjects.

Few graduates are now accepting Science posts in secondary modern schools. Here the subject is quite often taught by men and women without specialist training of any kind and the services of teachers who have studied Science as a main subject in college are at a premium. In a survey of the numbers of men and women entering training colleges in October 1957, just published (*Recruits to Teaching*, M. LAND: Liverpool University Press. 7*s*. 6*d*.), it is estimated that in a whole year's entry to the colleges less than a third of the men are taking science and mathematics, and in the case of women the figure is less than a sixth. Many children in modern schools get no Science lessons at all—either because the necessary staff is not available or because laboratory facilities are lacking. The latest report of the Science Masters' Association, *Science Teaching in Secondary Modern Schools* (1*s*., post free), makes the position depressingly clear.

There are those who still believe that salary increases alone will solve the problem. They are deceived. Science teaching will only attract the men and women who are needed when it offers the satisfactions of a job well done; when it is seen to be a worthwhile and a productive occupation in relation to the majority of boys and girls. The conditions under which Science is taught in many secondary modern schools cannot give these satisfactions, but the trouble is not confined to such schools : the failure of grammar schools to capture the interest of the C-streamer is notorious. The approach that is in some ways so successful with the future candidate for a university place fails utterly with others who are yet in the top quarter of the intelligence range. Some of the latter ' drop ' Science early; some fail at O level; and even a Pass in Physics or Chemistry is frequently of little lasting significance to its possessor—he says goodbye to the subject without regret.

These difficulties will not be removed by the introduction of a lower Pass level in the General Certificate of Education, any more than G.C.E. will prove the answer to the problems of the secondary modern school. The academic character of G.C.E., proper to its purpose, has little in common with the interests of most of the nation's children. Of the 10 500 pupils in attendance at secondary modern schools who offered subjects at Ordinary level in the 1958 Summer examinations, nearly half (4950) passed in not more than one subject and of these over 2000 failed entirely. There is now substantial evidence that local and regional examinations, tailored to meet the needs of the schools, can serve as a very powerful incentive to remain at school to many able youngsters for whom an approach through words and books alone would have little meaning; but the success of this development is absolutely dependent upon recognition of the fact that teaching methods must take full account of the ways in which such pupils learn.

The Association of Scientific Workers, in a recent policy statement, has recommended a scheme to second scientists and technologists from the Civil Service and industry to teach in schools, as an emergency measure. The idea is well-intentioned but action of this kind could at best serve only as a palliative. What the schools need is men and women who are able to *teach* science, and neither the Civil Service nor industry can guarantee that. The practising teacher knows, and the potential candidate for teaching may fear, that he will have much to do with boys and girls who have little appetite for the old text-book round. It is of course true that industrial efficiency might not be sensibly reduced were many of the graduates now filling industrial posts diverted to teaching. Pious appeals to employers are unlikely to alter existing practice, but in any case something much more radical is required.

It is from the grammar schools that the great majority of teachers are recruited, whether they come via university or training college, and it is in the grammar schools that reform must begin. Is it unreasonable to ask that a campaign should be launched there to recommend Science teaching as a career ? The Crowther Report has this to say :—

> Teaching has not presented itself to them (i.e. mathematicians and scientists) as the obvious thing for them to do. They have to be shown (and can be shown) that it is at least one obvious thing to do, and one which has many advantages if their interest in their subjects is on a broad front. Opportunities for private study, for writing, for contact through professional associations with others whose interests lie in the same field, for travel and for acquiring such tools of one's trade as books or scientific apparatus . . . these are attractions which teaching can offer.

The Ministry pamphlet, *Science in Secondary Schools* (H.M.S.O. 1960, 6*s*.), attacks the question in another way.

> It is too often assumed that anyone can teach . .. and that the reason why science is included in the curriculum of schools is primarily the need to produce an adequate number of research workers . . . but teaching too needs its quota of first-class brains...and...presents problems no less urgent and requiring no less ingenuity for their solution than many of those to be found under the heading of scientific research.

It is time indeed that sixth formers should be made to feel that teaching can offer scope to men and women of the highest talent. It is time too that they were accurately informed of the reasons for the inclusion of Science in the time-table in secondary schools of all types (the more thoughtful ones might well query theory and practice). To say that young people respond to challenge is a favourite gambit on Speech Day; but do we challenge them? It is almost certainly true that teachers have often allowed their own dissatisfactions to stand in the way of their pupils in this matter. Too often the word and the attitude that could have turned the scale have been missing. What right has anyone to deny so great a vocation to any boy or girl?

Yet more than all the inducements that the teacher can offer is the pupil's own experience of the subject as it was presented to him. Was the method of science, as it is understood in research laboratories, exemplified by the treatment of Science in school? Had the student any opportunities at all of carrying out for himself a real investigation? Did he learn to use his hands and his eyes and ears, and become convinced of the basic role of first-hand observation? Was the subject-matter of his course clearly related to things as he saw them in the world outside? Did it make any contact with the philosophy and history of a subject which is nothing if not up-to-date? Unless boys and girls can be prevented from coming to think of Science merely as a body of knowledge (and co-extensive with an examination syllabus at that), the dreary round will not be broken. In Mr. Land's survey, referred to above, an effort was made to assess the attitudes of students entering college towards the main subjects of the curriculum. In the case of women, Mathematics, Chemistry and Physics filled the last three places in respect both of ' interest value ' and ' social importance ': " the consistent lesson of the girls' grammar schools is the futility of Physics and Chemistry ".

This year the three-year training college course begins. Neither the universities nor the colleges will produce the teachers that we need unless they are helped by the schools.

The Parliamentary Secretary to the Ministry of Education, Mr. Kenneth Thompson, has clearly indicated a major change in teacher-training policy. At the moment only about 35 per cent of all secondary school teachers are graduates, but the rapid expansion of the universities will mean that more and more graduates will enter the schools. On the Arts side, indeed, we can look forward to the time when the graduates recruited to teaching will be sufficient to fill all the vacancies in secondary schools. The same is unlikely to be true in the field of Science and Mathematics however and there will remain a need for training colleges to supply additional teachers in these subjects as well as teachers for primary schools and specialists in subjects for which universities do not cater—handicraft, physical education and housecraft. Mr. Thompson has therefore advised the training colleges that the time has come to halt the bias in favour of general secondary training and to concentrate, so far as the secondary field is concerned, on the training of specialists. Here is a revolution, much to be welcomed. But it is one that forces immediate re-thinking of what the course of Science teacher training should be.

The first essential is surely that students-in-training, whether in universities or training colleges, should be alerted to the fact that most boys and girls in secondary schools—grammar schools not excepted—do not thrive on a programme of learning Science for learning's sake. The key factor for success with these pupils (as with all pupils) is interest—but in their case it is predominantly interest in the applied science of the present day. Principles must be approached through practical situations in which the pupil plays a real part. Enthusiasm is aroused by reference to matters that relate to the pupil's everyday life, both now and later when he takes a job. This is not a matter of merely selecting juicy tit-bits of information, and it is no job for the amateur. It demands a thorough understanding of the aims of Science teaching, an extensive knowledge of the subject and much technical skill.

It should be the aim of any college or university course in Science that is considered appropriate as a preparation for teaching to pursue studies along a fairly broad front—dropping none of the three main branches altogether—while encouraging the student to delve deeply into some one aspect. Every student should have the experience of carrying out some significant programme of experimental investigation during his course, involving him in research into the literature and demanding ingenuity of mind and hand. He should certainly be carefully instructed in a wide range of laboratory skills and given solid guidance in matters of laboratory organization—subjects that have been generally neglected hitherto. We may well be reaching the position in this country where general degrees in Science should take specific account of these requirements of teaching, as the time approaches when a majority of science specialists will be graduates. So far as the training colleges are concerned, three years will be little enough in which to train a specialist along these lines.

Teaching offers less material reward than many alternative careers in Science, yet men and women of distinction are still to be found in the schools. More will be found there when Science is taught in a way that commends it as a study to a greater proportion of boys and girls. But there are other means that could be used at once to increase the attractiveness of Science teaching, and of these the most effective would be the provision of laboratory assistance in all secondary schools. Another would be the recognition that practical work in Science cannot be carried out in groups of thirty or more and that half-classes are as essential here as in the case of handicraft or housecraft lessons. The apparent anomaly that this would require *more* staff can be discounted when one considers the real productivity of existing practice!

The test of good teaching must in part be the use that boys and girls make of it in adult life. If teaching is stimulating it will bear fruit—and one of the fruits of science teaching should surely be a supply of Science teachers. Perhaps we ought to measure our success as teachers not by the number of G.C.E. passes at O level (with a careful forgetfulness of all those who did not sit), but by the number of those young men and women who come back into the schools to follow in our footsteps.

The Royal Society Tercentenary Celebrations

by L. F. BATES

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It is fitting, when a learned society has endeavoured for a period of three centuries to ' encourage philosophical studies, especially those which by actual experiments attempt either to shape out a new philosophy or to perfect the old ', to quote the words of its founder, King Charles II, that it should rejoice and invite the savants of many lands to share in the rejoicings. It is also fitting that it should attempt to give the world an account of its doings and to show, by means of special lectures, by visits to industrial, scientific and university laboratories and other centres of culture, and by broadcasting on sound and television, how great has been its impact on our life and times. The Royal Society in celebrating its tercentenary made the most of a great opportunity.

Every participant in the Celebrations must have come away with a feeling of great admiration for the wonderful feat of organization which he had been privileged to experience. The Officers of the Society and the whole of their Staff deserve the warmest thanks for the extraordinary smoothness with which the Celebrations proceeded. From Registration to final Banquet there appeared never to be the slightest hitch of any kind.

The Formal Opening by the Royal Patron was an unforgettable occasion, and there were many who wished that colour television of the splendid scene in the Albert Hall had been possible. Over four hundred Fellows of the Society and about two hundred and fifty representatives of national academies of science, universities and international scientific organizations were present to hear the Queen speak of the responsibilities of the Society and of the growing field of international scientific co-operation, and to see her present a silver bell to the Society as a token of her continuing interest in the work of the Society.

During the Celebrations the rooms of the Society were adorned by very many messages of congratulation; at the Formal Opening, Professor D. Graffi of the University of Bologna, Professor J. Lecomte of the Academy of Sciences of France and Sir John Eccles, F.R.S., President of the Australian Academy of Science, delivered messages of congratulation, while King Gustav VI Adolf of Sweden addressed the Assembly following his admission to the Fellowship. One noted in the interesting Tercentenary Address by Sir Cyril Hinshelwood, O.M., P.R.S., his striking comparison between the perils of atomic warfare and the prospects of eternal damnation feared by many in an earlier age, his reference to useless and unjustified timidity, and his conclusion that the clear task of science is to go forward undeterred by the uncertainties of our times.

Another memorable occasion was the Ceremony of the Conferment of Honorary Degrees by the University of London, an occasion which also served to mark the centenary of the granting of the first degrees in science by the University, for as Queen Elizabeth the Queen Mother, The Chancellor, mentioned in her Address, in 1859 the Senate established examinations for the degree of B.Sc., but it was in 1860 that the first three candidates satisfied the examiners. Honorary Degrees in Science were conferred upon King Gustaf VI Adolf of Sweden, Professor H. J. Bhabha, Professor Detlev W. Bronk, Sir Macfarlane Burnet, Professor G. C. de Hevesy and Sir Thomas Merton, and many of us will remember for a long time the fine addresses of the Special Orator (Sir Patrick Linstead) and the Public Orator.

A large party visited Oxford on July 21st, when the University conferred Honorary Degrees upon Professor Felix Bloch, Dr. A. N. Richards, Professor N. N. Semenov, Dr. E. W. R. Steacie and Professor Öjvind Winge; subsequently, the Warden and Fellows of Wadham College, the College so intimately concerned with the early history of the Society, gave a Garden Party for the visitors.

A like visit was paid to Cambridge on July 25th when the University conferred Honorary Degrees upon Sir John Eccles, Professor S. O. Hörstadius, Professor B. A. Houssay and Professor J. H. Oort, the ceremony being followed by a garden party in Trinity College and St. John's College given by the Masters and Fellows of the colleges.

Receptions were held in London by Her Majesty Queen Elizabeth the Queen Mother for the wives of official representatives and overseas Fellows at Clarence House; by Her Majesty's Government at Lancaster House, where Viscount Hailsham, Minister for Science, received the guests; by the University at Senate House; by the Lord Mayor and Corporation of the City of London at Guildhall; by the Twelve Great City Companies at the Mercer's Hall; and by the Parliamentary and Scientific Committee at the Houses of Parliament. It was very pleasing to note the interest which industrial undertakings showed in the Centenary Celebrations. In particular, one appreciated the Film Première at the Royal Festival Hall, arranged by the Shell International Petroleum Company. Here, participants saw, in addition to a film of the opening ceremony, two new scientific films: ' The Revealing Eye ', a survey of scientific phenomena which because of their speed, their slowness or their size escape observation by the human eye but can be recorded on moving film, and ' A Light in Nature ', a view of world science at the frontiers of knowledge and of its 300 year old heritage of observation and thought. (Incidentally, the Shell Company will be pleased to loan these films for exhibition to non-paying audiences.)

The Society had organized a series of very interesting lectures, and one hopes that these, together with the Tercentenary Address, will be published in due course and widely read. The series comprised a lecture on ' The Chromosomes and the Theory of Heredity ' by Professor C. D. Darlington, F.R.S., ' The Evolution of Nuclear Power Plant Design ' by Sir Christopher Hinton, K.B.E., F.R.S., ' The Problems of Transplantation ' by Professor P. B. Medawar, C.B.E., F.R.S., ' The Physics and Chemistry of Nervous Conduction ' by Professor A. L. Hodgkin, F.R.S., ' The Study of Nuclear Interactions at Very Great Energies by Professor C. F. Powell, F.R.S., ' New Horizons in Organic Chemistry ' by Sir Alexander Todd, F.R.S., ' Molecules in Crystals ' by Dr. Dorothy Hodgkin, F.R.S., ' The Investigation of the Universe by Radio Astronomy ' by Professor A. C. B. Lovell, O.B.E., F.R.S., ' The Metamorphosis of Insects ' by Professor V. B. Wigglesworth, C.B.E., F.R.S. and ' Trends in Aeronautical Science and Engineering ' by Sir Arnold Hall, F.R.S. The Conversazione on July 23rd was, naturally, on a very large scale, and the Society availed itself of the kind facilities of the Royal Academy of Arts to stage a special tercentenary exhibition of items of scientific research in progress in this country, and of the privilege of viewing the Summer Exhibition of Art.

The participants had a wonderful choice of excursions of special interest. There were whole-day visits to Canterbury and Greenwich (including the Royal Naval College and National Maritime Museum), St. Albans, the John Innes Horticultural Institution, the National Institute for Research in Nuclear Science, the Associated Electrical Industries Research Laboratory, the Atomic Energy Research Establishment, the Royal Greenwich Observatory, the East Malling Research Station, the Bradwell Nuclear Power Station of the Electricity Generating Board, Whipsnade Zoological Park, Leith Hill Place and Polesden Lacey, the National Institute for Research in Dairying, Rothamsted Experimental Station, the Royal Aircraft Establishment, the Plastics Division, I.C.I. Ltd., Imperial College, the British Petroleum Research Centre, and the General Electric Company Research Laboratories. A visit of special interest was that to Woolsthorpe Manor, Newton's birthplace, and to the King's School at Grantham; in the course of the visit a plaque on the wall of the Old School where Newton was a schoolboy was unveiled, and a wreath was placed on Newton's statue at St. Peter's Hill in Grantham on behalf of the Royal Society, by Professor E. N. da C. Andrade, F.R.S. In addition, there were about thirty visits of a half-day duration.

A Special Service was held at St. Paul's Cathedral on July 24th, when the Dean of St. Paul's, the Very Reverend W. R. Matthews, preached a special sermon relating to Sir Christopher Wren, Past President of the Society and architect of the Cathedral. At the Tercentenary Celebrations Banquet, at Grosvenor House on July 26th, the Prime Minister proposed the toast of The Royal Society, to which the President replied; Lord Adrian, O.M., F.R.S., proposed The Guests and Dr. F. C. James, Vice-Chancellor of McGill University, and Professor A. H. T. Theorell, For. Mem. R.S., Stockholm, responded.

The tercentenary revels are ended, the members of the great congregation of scientists have dispersed to their several countries, but there abides with us here in England the satisfaction of a great, useful and historic meeting superbly planned and held in a manner truly befitting the Royal Society of London.

The Author :

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Medical Electronics
Third International Conference and Exhibition

by V. T. SAUNDERS
Uppingham School

This Conference and Exhibition, organized by the Electronics and Communications Section of the Institution of Electrical Engineers in association with The International Federation of Medical Electronics took place at the Olympia Exhibition Hall, London, during the period 21–27 July 1960.

1. The Conference

In the Section ' Instrumentation for Medicine and Biology ' Professor J. McMichael lectured on ' Instrumentation in Medical Practice and Research '. Perhaps the foremost advance of medical instrumentation is that discussed in the papers ' Medical Applications of Electronic Computers ' (L. B. Lusted, and R. S. Ledley) and 'An Approach to the Solution of the Human Bio-Thermal Problem with the Aid of an Analogue Computer ' (C. H. Wyndham and A. R. Atkins). We can nowadays accept the idea of an electronic device that stores the past medical history of a patient, records the current symptoms, and delivers the conclusions drawn therefrom.

The lecture ' Prospects in Medical Electronics ' (Dr. V. K. Zworykin) raises now and for the future the question as to whether there will be forthcoming, in the presence of the insistent demands of industry and the Services, an adequate supply of young men sufficiently richly endowed with the enquiring mind and also strictly trained in the discipline of the scientific method to carry on the work already started.

The subject dealt with in one of the general sessions was ' Medical Electronics in Aviation and Space Flight '. During the accelerations now met with in high-speed flight, inertial forces are set up which affect the various organs of the body, e.g. it may happen that the heart will fail to pump an adequate supply of blood to the brain and there is a consequent loss of vision followed by unconsciousness.

Flight conditions also affect the communication system between the senses; for instance, the brain and the muscles may change their characteristics, and the sense organs themselves may give false information. Investigations in this field require various delicate measuring techniques familiar to the neurophysiologist, who also makes an important contribution to Aviation Medicine.

There are other problems too. For instance, modern flying may take place in conditions which produce loneliness and boredom. How can these factors be kept under control and a high standard of vigilance maintained? What is the effect of continuous vibration on aircrew performance? And in particular, when all the relatively minor short term discomforts operate as one long but not very easily defined combined stress, what does it do to the airman and what can be done to minimize the hazards? The solutions to all these questions will undoubtedly involve electronic devices.

Whereas at the beginning of this century physicists were concerned about the effects on the human body of x-rays, ultra-violet light and gamma rays, in the mid-century investigations are being made on the biological effects of intense radio-frequency radiation, measurements of potentially hazardous electromagnetic fields are being carried out and the industrial and medical effects of microwave radiation are being assessed.

In a science rich in new and exciting words the heading ' Ultrasonics ' has an almost archaic ring. Nevertheless the lecture ' The Diagnostic Use of Ultrasonics ' and the paper ' The use of Ultrasonics in Medical Diagnosis ' show that the long practised but still often baffling art of diagnosis is calling new tools to its aid; one speaker was heard to say that these ultrasonic waves bounce off soft tissue.

In 1925 the Royal Society established a gallery of scientific exhibits in the British Empire Exhibition at Wembley at which the present writer acted as demonstrator and lecturer. Under the name of Dr. E. D. Adrian there was shown an electrocardiograph arranged for recording photographically the current produced by the beating heart, the limbs of the subject acting as leads from the heart to the Einthoven string galvanometer which was described as having a pointer so light in weight that it could follow the most rapid changes in current with the minimum of distortion. Thirty years later the pointer consisting of an electron stream is infinitesimal in weight in comparison with the silvered glass thread suspended in the magnetic field. The 1925 cardiograph might have been described as a sizeable piece of furniture whereas the 1960 instrument in its electronic form is portable.

Newer developments such as vectorcardiography facilitate diagnosis, and modern instruments can be used for phonocardiography, the recording and analysis of heart sounds, a development of the traditional and still indispensable stethoscope which however may now be an electronic one. The paper on this subject came from Japan.

Ballistocardiography is another aid to diagnosis in which electronics plays a vital part. Here the patient lies on a freely suspended bed and the ' recoil ' which occurs each time his heart beats, as blood is ejected from the ventricle into the aorta, is recorded and analysed, in terms of displacement, velocity and acceleration.

The present state in this branch was dealt with by Professor V. V. Parin (U.S.S.R.) in his lecture ' The Development of Ballistocardiographic techniques in the U.S.S.R.'

From the U.S.S.R. too there came a contribution ' Some Aspects of the Problem of Bioelectrical Control of Medical Appliances '.

When an impulse travels along a nerve fibre it is accompanied by a sudden rise of potential, inside the nerve, of about 100 mv; the nerve fibre recovers and the potential falls in about 10 msec. The same kind of thing happens to a motor fibre but, in addition, the electrical wave is accompanied by a mechanical contraction or twitch of the fibre.

Enormous advances have been made in our understanding of the behaviour of motor and nerve fibres by the study, through the use of electronics, of their associated electrical phenomena. Microelectrodes can be inserted into tissue so as to record the firing of single fibres; gross electrodes placed far from the active fibres can pick up signals of the order of micro-volts; high quality amplifiers have been developed specially for biological work; special-purpose computers have been developed to analyse massive information and present it in a less redundant form.

One practical outcome of the discovery of these bio-electromotive forces is the work of Nightingale and others on myo-electric control of an artificial muscle, and, at the Conference, Brigadier Swettenham made a preliminary report on the problems of powered limb prosthesis. The Science of Electronics is making great contributions to the work at the Limb Fitting Centre at Roehampton and at the Polio Centre at West Hendon.

Servo systems and automatons are now common in electronics. A ' servo-anaesthetizer ' has been devised, at the Mayo Clinic, which is operated by the activity of the patient's brain; the activity is detected electronically and through the use of relays the rate of delivery of the anaesthetic is either increased or decreased. Is this a true example of the ' prescient automaton '?

In poliomyelitis the breathing muscles may be partly paralysed, and the patient's respiration may then have to be assisted by a machine. Electronic devices can now measure the patient's disability, and control the degree of assistance provided by the machine. In this way the volume of breathing is automatically adjusted to suit the need of the individual patient. This must be an example of the ' corrector automaton '.

When the proceedings are available physicists will want to see 'A Strain Gauge for Physiological Use ', 'A Specification for a General Purpose Biological Amplifier ', ' Nuclear Magnetic Resonance Blood Flow Meters ' and ' The Ultraviolet Colour Translating Television Microscope and its Applications '.

2. The Exhibition

The exhibition was international in character; nearly half the stand space had been booked by overseas electronics manufacturers and in particular Japan was strongly represented by a contingent of 15 firms from that country's new-formed electronics industry.

In a first walk round during which the writer avoided becoming submerged by the details of any particular equipment the common features of all these new aids to diagnosis and recovery revealed themselves. First there is the signal set up by the patient—a temperature, a pressure, a chemical state, a repeating pattern, a colour change—and this is introduced to the electric circuit by a transducer; then comes the familiar electronic amplifier; finally the output of the amplifier is shown on screen or dial or moving graph paper, or is stored for subsequent examination.

During the first tour, too, the two quite distinct parts into which the exhibition was divided were clearly seen. In the Research and Hospital section research organizations and hospitals were demonstrating various pieces of experimental equipment; here was to be seen the work of men envisaged by Huxley when he wrote " the highest object that human beings can set before themselves is not the pursuit of any such chimera as the annihilating of the unknown; but it is simply the unwearied endeavour to remove its boundaries a little further from our little sphere of action ". In the Manufacturers' Section there was to be seen the wide range of equipment which has already passed through the experimental stage and is now in production for daily use.

An impactive feature of the Research and Hospitals Section was the equipment now available for the continuous monitoring of the stages of child-birth; the graphical charts of the heartbeats of the mother and of the still unborn child showed how a continuous but remote watch can be kept on the patient.

Moving on a pace or two the writer found himself in what must be described as a small crowd, seeing, handling and hearing about the radio pill (designed by the Bioengineering Laboratory of the Medical Research Council and manufactured by an English firm); it is 16 mm long, 6 mm in diameter, and it contains a radio transmitter which has a range of about 10 feet. The makers describe it as " a sub-miniaturised, encapsulated radio-frequency tranducer, working at a nominal frequency of 400 kc/s, the frequency of oscillation being caused to change as a function of the particular medical parameter being investigated ". The pill is powered by a single cell, 3 mm. thick, having a working life of 36 hours at a power of 10^{-3} watts. A loop aerial placed close to the patient picks up a sufficiently strong signal to make the system relatively insensitive to interference; the output of the receiver is fed into a pen chart recorder. The transmitter is switched on by a self-contained inertia switch which is operated by shaking the pill before swallowing it. It has been suggested that the pill, or capsule, will find a use in the food processing industry where, during cooking, continuous records of temperature, pressure and pH measurements are important.

Another interesting feature in the Research Section was the optical chopper photometer designed and made in the Physics Department of Guy's Hospital Medical School to one of the members of the staff of which the writer is indebted for the following description. This exposure meter for high-power micrography is designed to measure the mean intensity of illumination in the plane of the photographic film, under conditions where a high-power objective, heavily filtered light source and an optically dense specimen prohibit the use of simple photometers. The system consists of a selenium barrier layer cell placed in the plane of the film and connected to the input of a low noise a/c coupled transistor amplifier. The light source is interrupted at about 75 c/s by a rotary disc, the resulting 75 c/s signal from the photocell being amplified and measured by means of a transistor voltmeter. The light intensity is read from a meter in the output circuit calibrated in arbitrary units.

On the manufacturers' stands there was so much that held the interest and attention of the visiting physicist that it must be accepted that the selection dealt with here is entirely personal to the writer who lays no claim to be reporting on the most important features of the exhibition.

On 23rd May 1960 a twelve-inch image amplifier, made by an English firm, was officially inaugurated at the King Edward VII Hospital at Hertford Hill, near Warwick. One of these equipments was set up in the exhibition and the description of its design and purpose given here is drawn from notes supplied by the makers. With the conventional system of x-ray viewing the light output from the fluorescent screen is low and the image must be observed in darkness; a radiologist who is to make a visual examination of a patient must spend anything from twenty minutes upwards in accustoming his eyes to darkness. The successful observation of the often murky image depends almost entirely on the condition of the human eye. When the light level is low the visual acuity is often too poor to see all the details or to discriminate between surfaces of differing brightness. The image amplifier substitutes for the eye a more powerful lens system and an electronic eye. The light from the x-ray screen is reflected by a mirror through a wide-aperture lens system to an image orthicon camera tube. The visual information has now been converted into electronic information and therefore it can be amplified, so the brightness and the contrast can be greatly increased. The resulting twelve-inch diameter picture of the image is viewed on a television monitor—or a number of remote monitors—in which the 1000-line triple interlace system provides a high-definition image. The picture is life-size and large enough to include the major organs of the body. The examination can be carried out in a normally lighted room and the patient undergoes less radiation than in the ordinary methods of the past. A ciné-camera can be attached and a continuous record of the large fluorescent images made so that the results of the examination are available for reference purposes; this can be done without in any way interfering with the visual examination and no additional x-ray dosage is incurred.

On the stand of one of the English companies there were to be seen a very wide range of specialized valves and tubes including direct view storage tubes, oscillators and amplifiers for diathermy equipment, high power magnetrons and klystrons for linear accelerators, image orthicon and vidicon television camera tubes, image intensifiers, glow modulator tubes, rectifiers and cold cathode tubes. The direct view storage tubes, with electrostatic focus and deflection, can display single or repetitive transients with duration ranging from milliseconds to minutes. It is claimed by the makers that they should be of interest in electrocardiography, electroencephalography and radiography. In addition to storing waveforms these tubes can also store half-tone pictures and so a single frame can be selected from a televised x-cineradiograph and stored for later examination. A prototype storage device was shown which selected and stored a single frame of a television type picture; the storage can be about a week, the time of display several hours, but the picture may be erased instantly at will. This device is likely to be of use in teaching hospitals where a particular phase of a major operation could be frozen on the monitor, for further explanation or discussion by the instructor, whilst the operation continues.

" Colour T.V. in Medicine " was a heading which excited interest. The makers of this equipment had two exacting demands to meet: (i) the faithful rendering of colour and (ii) keeping the size down to such an extent that it could be accepted in an operating theatre where space is always at a premium. There was a demonstration of flesh tissues, green theatre towels, gleaming surgical instruments and brown rubber gloves shown on a 21-inch colour T.V. receiver. It was stated that many surgical operations and clinical procedures have been shown on a screen 7 feet by 4 feet and that packed audiences have seen anatomical experiments taking place under a microscope.

A young man exhaling into a face mask joined by a flexible tube to a piece of equipment about the size of a kitchen gas cooker caught the visitor's attention. This was the Ird-o-meter—a respiratory carbon dioxide analyser, of which the makers supplied the following description. Two hot wire spirals (the radiators) project infra-red radiation through two tubes, the analysis and reference tubes, sealed by windows transparent to the infra-red wavelengths being used. The radiation beams are alternately exposed and cut off by a chopper vane rotating at a frequency locked to that of the a.c. mains supply. After the beams have passed through the tubes they fall on a detector unit, which consists of two chambers in pressure connection together, filled with a pure sample of carbon dioxide and separated by a thin diaphragm which forms one electrode of a small electrical condenser. The radiation transmitted through the analysis and reference tubes is absorbed by the gas in the two detector cavities, causing an increase in temperature and therefore in pressure. If the transmission through the analysis and reference tubes is identical the diaphragm remains stationary; on the other hand if a sample of gas which absorbs radiation of the same wavelength as that in the detector unit is introduced into the analysis tube the transmission through the two sides becomes unequal and the diaphragm undergoes a displacement whose amplitude is related to the concentration of gas in the analysis tube. The displacements of the diaphragm produce changes in electrical capacitance of the condenser, which are converted to current changes by applying a polarizing potential, then amplified and fed to the meter and recorder.

At a Friday Evening Discourse at the Royal Institution in 1931, Professor F. L. Hopwood, Physicist to St. Bartholomew's Hospital, demonstrated some of the properties of inaudible sound; he showed that small fish could be killed and that the texture of certain water plants could be destroyed; it was subsequently established that ultrasonic waves of a certain wavelength and of sufficiently high power will kill bacteria. It was not surprising therefore to find, in this exhibition, a " Large Scale Ultrasonic Cleaner " consisting of a tank of stainless steel suitable for any of the normal cleaning agents. The ultrasonic waves are set up by a bench-standing generator with a peak output of 500 watts at 40 kc/s. To eliminate dead spots the waves are fed into the liquid by twelve transducers arranged to cover the whole of the base of the tank. This cleaner has been designed for the rapid and efficient cleaning of surgical instruments but it is also claimed that it is an effective means of removing radioactive contamination from delicate mechanisms which might be damaged by scrubbing.

Also on show was a gamma alarm monitor designed to give warning when background x-ray or gamma radiation rises above the permissible level. For industrial use the monitor provides a means of complying with the draft Factory Act relating to the use of sealed radioactivity sources. In low-level counting laboratories it will give an immediate indication of a sufficient rise in the background gamma level and in hospitals, isotope laboratories and nuclear energy plants its alarm and monitoring facilities can be used to control the exposure of personnel to ionizing radiation below the maximum permissible dose level.

The monitor consists of a halogen quenched Geiger-Müller tube, a ratemeter and an alarm circuit with an adjustable trip which may be pre-set at any level within the range 0·05 to 10·0 milli-roentgens an hour. Any increase of background radiation above the pre-set level causes a red alarm lamp to be switched on, giving clear warning of danger. Due to the use of transistors in the ratemeter and alarm circuits the power consumption is only 1·5 watts; the dimensions are 9 in. × 9 in. × 5 in. and the total weight is 9 lb. The instrument may be used suspended upside-down from the ceiling or stood on a bench.

On the same stand also there was a series of unit-built hand and clothing monitors designed for the rapid detection of radioactive contamination on the hands and clothing of persons handling radioactive materials. An all purpose equipment consists of one pair of alpha hand units, one pair of beta-gamma hand units, an alpha clothing unit and a beta-gamma clothing unit. Alpha counts are made by photomultiplier scintillation counters and the beta-gamma counts by halogen quenched Geiger-Müller tubes. Detected contamination is indicated by a meter fitted to each unit calculated in terms of the maximum permissible level.

Amidst all this equipment for the professionals it was pleasing to find an item which will bring happiness to many ordinary people—a hearing corrector to be worn entirely behind the ear. It measures $2\frac{1}{2}$ in. × $\frac{3}{4}$ in. × $\frac{1}{2}$ in., contains earphone, amplifier, microphone, controls and battery and weighs only three-quarters of an ounce; the electric circuit, which contains 17 components, weighs $\frac{1}{15}$ oz., and measures $\frac{1}{4}$ in. × $\frac{5}{8}$ in. × $\frac{5}{16}$ in.; the battery has an average life of 80 hours. The gain within the frequency range 500 to 3000 c/s lies between 31 and 45 d.B. This is another instance of sub-miniaturisation.

3. The U.K.A.E.A. Exhibit

The Atomic Weapons Research Establishment of the United Kingdom Atomic Energy Authority displayed the following instruments, all of which have been developed to contribute to the protection of all people who work with radioactive substances.

3.1. *Simple Integrating Gamma Monitor*

The monitor uses a 10 cc air/argon-filled ionization chamber directly coupled in series with a Hammer dosimeter-type electrostatic relay (manufactured by Physikalisch-Technische-Werkstatten, GMBH, Freiburg, West Germany). Current collected by the ionization chamber charges the electrostatic relay and when a definite quantity of charge (equivalent to an integrated dose of gamma-radiation) has been transferred, the free plate of the electrostatic relay deflects and closes a contact. This initiates a relay sequence that enables the pulse to be recorded on a mechanical register (which can be reset) in fixed integrals of dose; for example, 10 milliroentgens or 1 roentgen, depending on the sensitivity of the ion chamber. Two ranges are available, 10 mr–100 r and 1 r–10 000 r, and either may be preselected by adjusting the ion chamber pressure to 0·3 atmosphere or 30 atmospheres respectively.

3.2. *The Phantoscope*

This instrument is a new analyser for rapid routine radiation investigation. It identifies radiation by the observation of characteristic radiation spectra. The X-ray spectrum of plutonium-239 is displayed on the cathode-ray tube by means of an X-ray proportional counter, and the gamma spectrum of uranium-235 is displayed by means of a sodium iodide scintillation probe.

The analysis is effected by sweeping a window through the pulse distribution of the particular counter, coincident with the time base of the display. Count-rates of up to 104 c.p.s. and time bases varying from a few seconds to several minutes, may be used.

3.3. *Measurement of Plutonium-*239 *in wounds*

The plutonium monitor type PNI 1080, developed for A.W.R.E. by Plessey Nucleonics Ltd., is designed to detect minute amounts of plutonium-239 which have become lodged in wounds. With appropriate recalibration it will also detect americium-241 (59·8 Kev gamma emission) and uranium-235 (184 Kev gamma emission).

The detector probe consists of a thin sodium iodide scintillator mounted on the end of a 28 mm diameter photomultiplier tube. The probe unit also contains the dynode resistor chain as an encapsulated unit. A set of caps having central holes of different diameters is provided, and these may be clipped over the phosphor to provide collimation of the incident radiation.

The instrument's sensitivity to direct radiation from plutonium is 15 counts/sec/microcurie. Background is about 2% of this figure. The maximum permissible body burden of plutonium (0·04 μc) located at one point will give about five times background count through 1 cm of human tissue.

3.4. *Measurement of Plutonium-*239 *contamination of surfaces*

The degree to which a surface is contaminated by plutonium can be determined, using this instrument, by measuring the X-rays which the isotope emits. (Plutonium-239 emits not only X-rays but also gamma rays and alpha particles, and formerly contamination was measured by detecting the alpha particles; the present method offers several advantages, however.)

A simple X-ray probe unit is used with a standard survey meter that has been modified for this application. The probe consists of an EMI photomultiplier tube to which is attached a thallium-activated sodium iodide crystal 1·5 inches in diameter by 1 mm thick. A simple modification of the survey meter's circuit limits the effective response of the instrument to photons in the range 10–25 KeV, and this makes it relatively insensitive to unwanted background radiation. In a normal gamma background (up to 10 micro-roentgen/hr) it will measure down to about 3 microcuries of plutonium-239 per square metre.

Plutonium-239 emits X-rays at different energies. The instrument is designed to measure three of these energy levels (13·6, 16·9 and 20·2 KeV), which occur in about 4 per cent of the alpha-disintegrations. These radiations will penetrate several metres of air as well as the light seal, and consequently the probe can be kept well clear of the contaminated surface. As compared with the alpha probe, therefore, which has to be used near the surface, it is less liable to failure through rupture of the light seal and less likely to become contaminated itself. Its rugged nature makes it particularly suitable for surveys outside buildings; for example, on concrete washdowns.

3.5. *Blood Cell Differential Counter*

This electrically-operated counter is used for indicating numerically the percentage of types of leucocytes identified in a differential leucocyte count. It is operated by the use of press-button switches each of which indicates a particular type of leucocyte. When the total number of leucocytes identified reaches 100, the register ceases to operate and shows the total number of each type of leucocyte. The numbers so registered can be cancelled by the use of a single stroke lever. By the use of a switch, totals exceeding 100 can be indicated.

3.6. *Miniature Transistorized Counters*

(*a*) *The transiscaler:*

This is a self-contained instrument of low power consumption and complete portability; it is particularly suitable for use in road vehicles. It can be used with alpha scintillation counters and with end-window GM beta counters for field measurements. The counter will operate off the typical automobile battery voltage of 12 volts, with positive earth, or, indeed, off any internal or external battery supply of 11·5 to 26 volts.

(*b*) *Universal alpha and beta laboratory counter:*

This counter consists of four basic transistorized units: an amplifier, a scaler, a timing unit, and a power unit. Each unit is self-contained and measures 10 × 4 × 6 inches; each is designed to operate on a 20 volt supply but also contains power stabilizing to enable an external 24 volt DC aircraft or vehicle supply to be used.

3.7. *Measurements of Iodine*-131 *in human and animal thyroids and in milk*

The equipment consists essentially of a gamma scintillation counter for measurements on human thyroids, and a well crystal spectrometer for measurements on animals and milk. Both are intended for use in an emergency.

When radioactive iodine is taken into the body it concentrates mainly in the thyroid gland, the maximum permissible quantity in the organ being 0·14 microcurie (according to the Recommendations of the International Commission on Radiological Protection, 1959). This apparatus, which has been in use at A.W.R.E. for some years, consists of a suitably collimated sodium iodide gamma scintillation counter feeding into a conventional commercial linear amplifier, single channel analyser, and scaler. Its sensitivity, set up to embrace the 364 Kev iodine-131 line, is such that a count rate of ten times background is obtained for 0·1 microcurie of iodine-131 in the thyroid. Some statistical advantage is obtained if both the 284 Kev and 364 Kev iodine-131 lines are embraced.

The MRC Committee on Protection against Ionizing Radiations recommends that the maximum permissible quantity of iodine-131 in milk should be 0·065 microcurie per litre. The well crystal spectrometer, complete with pulse amplitude analyser, is suitable for assaying this isotope both in milk and in animal thyroids. It can also be used for the measurement of gamma-emitters such as caesium-137 and sodium-24.

In addition to the main equipment a miniature, transistorized version was on show. This, also, is suitable for the analysis of any gamma emitters, and complete spectra may be drawn with the aid of the commercial portable potentiometric recorder.

The Author :

Vivian Saunders was a scholar of Exeter College, Oxford, where he graduated in 1915. He was on the staff of the Ordnance College, Woolwich, and the Royal Naval Colleges at Osborne and Dartmouth, before joining Uppingham School, where he was in charge of the physics until 1954, and subsequently careers master.

ESSAY REVIEW

Reports on Progress in Physics, Vol. 23. (The Physical Society, London, 1960). [Pp. 629.]

The appearance of the twenty-third volume in this well-known series would serve to draw attention, if this were necessary, to the increasing amount of specialization within the field of Physics. The time has long passed when a physicist beyond the age of thirty could reasonably be expected to have any but the most superficial acquaintance with every one of those topics which, to some other physicist, would form the most interesting branch of the subject. The problem has already arisen of the extent to which a research student aged twenty-three should be obliged even to try and maintain some breadth of outlook, while the recent report on " The Postgraduate Training of Physicists in British Universities" (The Institute of Physics, 1960) takes it as almost axiomatic that " it is no longer possible to include in the three years of an undergraduate course all the fundamental knowledge which the more able future professional physicists should acquire by formal study ".

Many who are concerned with the teaching of physics have been considering what, if anything, can and should be done about the situation that is developing. In addition to the suggested four-year degree course, and the post-graduate lectures for potential research workers which have been discussed, there remains the possibility that some day the course for the first degree will give up the struggle to cover " the whole of physics ". A student, even as an undergraduate, might be permitted to specialize in, say, nuclear physics, or theoretical physics, in much the same way as some specialization is permitted in organic chemistry, or physical chemistry. It may be that this time has not yet arrived, and it may even be that its arrival can be postponed for some years. But even in the situation which already exists the jargon of one branch of physics is becoming unintelligible to practitioners of another branch, and the need for review articles is growing year by year.

Periodicals such as *Contemporary Physics* perform a valuable service in helping to meet this need, but it would be a mistake to regard the volume under review as being of this character. The articles contained in it are written by specialists and, it would appear, are intended to be read by specialists. With one or two exceptions they are, properly, " Progress Reports ". It is nevertheless of interest to consider them also from the viewpoint of a non-specialist reader who is either in search of a rather more detailed treatment of some topic than is provided by a general review, or who is embarking on the study of a new branch and wishes to be reasonably up-to-date.

There are ten contributions in the 1960 Reports on Progress in Physics, which fall neatly into three groups, with one over. Three deal with branches of optics. " Photoelectric Photometry " by H. J. J. Braddick (22 pages) is a short article, clearly written by an experimentalist. The emphasis is throughout on the measurement of low intensities with as much precision as can be obtained. Available devices are described and their performance compared, the spectral range considered being from the far ultra-violet to the infra-red. The fact that the article is informative does not stop it being readable.

The same remark might be made about " Optical Properties of Thin Films " by O. S. Heavens (65 pages). About a quarter of the space deals with the theory of light transmission through multi-layer films, and bristles with algebraic expressions; about another quarter is concerned with methods of measuring film thicknesses and properties. By judicious skipping, even a non-specialist can learn much from these sections. The third part is concerned with the results of experiments on particular materials and is of less general interest, while the last section deals with applications to anti-reflecting systems, high-reflecting systems, interference filters etc.

The third member of this group is of a different character. It is " New Developments in Interference Spectroscopy " by P. Jacquinot (46 pages). It is likely to be much more limited in its appeal, not so much because of the lack of interest, but because of the somewhat abstract and mathematical approach. After dealing with new methods of using the Fabry-Perot apparatus, rather more than half of the space is devoted to a discussion of new methods of spectroscopy in which the light beam is modulated by continuous movement of some part of an interferometer, converted to an electrical signal by a photo-cell or similar device and subsequently analysed and recorded by various electronic devices. This would be a lot easier to follow if, at some point, the author explained clearly what the apparatus consisted of and how it was used: but it is well-known that an expert in one subject is often so close to the trees that he is unable to see the wood as others see it.

The second group of three papers relates to solid-state physics. We may begin with " Group Theory in Solid-State Physics " by D. F. Johnston (88 pages). The article is arranged in two parts; in the words of the author, the first develops the concepts, relations and principles of the subject while the second considers applications to solid-state problems. The author further says that he has two kinds of reader in mind, the new post-graduate student approaching the subject for the first time and the more knowledgeable reader seeking a convenient exposition of the applications to solid-state physics. In attempting to cater simultaneously for two mutually exclusive categories, it would appear that the author has failed to write a review article or report on progress of any kind, and has produced what is essentially a monograph, including one section of new and unpublished material. It is doubtless a very good monograph: even the present writer (who, for these purposes, may be classed as a new post-graduate student) was able to follow the exposition in Part I as far as time permitted him to pursue it. But the justification for including it in the present volume is not obvious. It could even be maintained that it is not physics at all, but a particularly esoteric branch of mathematics which has applications to physics. (" The basic concepts of group theory . . . are essentially abstractions from the external world ".) On the other hand it should be mentioned that there are those physicists who would defend its inclusion, and would wish, instead, to exclude the article by Heavens on the grounds that it was not physics, at all, but technology.

" Band Structure Calculations in Solids " by L. Pincherle (40 pages), is for the man who already knows something about the subject. After a brief two-page survey of the kinds of experimental evidence available, the main body of the article is devoted to a description, discussion and comparison of the methods available for carrying out the calculations. The emphasis throughout is on principles rather than on mathematical techniques, although the starting point is the assumption of the " complete validity of the concepts of band theory ". No consideration is given to the results of the calculations, since other recent articles have dealt with this aspect.

What might at first sight appear to be a companion article on " Experimental Analysis of the Electronic Structure of Metals " by A. B. Pippard (91 pages) somewhat belies its title. At no point is there any account of any experimental work, and the reader who is not already familiar with this work, must go elsewhere for his information. The author begins with a useful summary of the ideas underlying the so-called independent-particle model of a metal, and devotes most of his pace to a discussion of the theory of a number of types of experiment, all of fairly recent development and all capable of giving direct, if limited, information about the electronic structure of the material. The topics dealt with include the de Haas-van Alphen effect, anomalous skin effect, cyclotron resonance and magneto-acoustic effects, and the paper like the one previously mentioned is clearly a valuable survey for those already engaged on work in any related field. It is interesting that of the two papers it is the supposedly experimental one that queries the validity of the basic assumption underlying the model currently used to represent the properties of a metal in relation to the conduction of heat and electricity.

The third group of papers relates to nuclear physics. " Precision Measurement in Gamma-ray Spectroscopy ", by G. A. Bartholomew, J. W. Knowles and G. E. Lee-Whiting (91 pages) is a thorough and detailed survey of the four principal techniques concerned, i.e. those based on coherent scattering (" crystal reflection ") the photo-electric effect, the Compton effect and the pair effect. Each in turn is described, starting from elementary principles; the performance of each is assessed, with examples of particular instruments, and inter-comparisons are made where appropriate. No attempt is made even to mention the results obtained by such measurements. The article is very detailed and will be invaluable to any research worker embarking on this sea for the first time: at the same time, by omitting detail, a general picture of the present state can be obtained by the general reader.

" Orbital Electron Capture by the Nucleus ", by R. Bouchez and P. Depommier (58 pages), is for the most part for the specialist theoretician. The first twenty pages or so, setting up the background, could be useful to a non-specialist if he were willing to supplement them in places by following up the references given. There is then a sudden transition to the rarified upper levels of theory, from which the authors do not appear to return to the ground state during the lifetime of the article. The conclusion appears to be—if an ignoramus may be so bold as to attempt to state it—that the theory can be brought into reasonable line with experiment without introducing more than two kinds of neutrino: at one time four appeared possible. But, in addition to parity, we now have to deal with helicity and chirality.

The third paper on " ^{3}He Induced Reactions ", by D. A. Bromley and E. Almqvist (85 pages), might suggest that the division of physics into two discrete disciplines (nuclear physics and the rest) has already taken place. Strong evidence for this belief comes from the fact that, on asking a nuclear physicist for some help in understanding the language of the article, the present writer received the reply " I don't know anything about that branch". The authors first discuss the usefulness of ^{3}He nuclei as projectiles in experiments designed to understand the nature of the processes taking place in nuclear reactions, and follow this by a survey of experimental results obtained to date, together with suggestions of topics which are likely to be fruitful in future research. There is no consideration of experimental techniques. The article is a proper review of its own field and seems likely to live up to its authors' hope that it will be of value to those engaged in, or about to be engaged in, similar investigations. But it is unlikely to be of much interest to anybody else.

The tenth and last article is by M. J. Seaton and is on " Planetary Nebulae " (42 pages). Planetary nebulae are—to quote the author—large (10^{17} cm) diffuse clouds of ionized gas, mainly hydrogen, surrounding certain hot stars, and the purpose of the article is to account for the observed emission spectrum in terms of the processes taking place in a low-density gas not in thermodynamic equilibrium and exposed to ultra-violet radiation (from the star). The only concession made to the non-specialist is a short list of books on the same, or related, topics: the remainder is theory, and comparison of the theory with experimental results.

It is clear that, whatever the authors may have thought that they were doing, they have produced a series of progress reports, suitable for the most part to be read by specialists and of limited general appeal. Any serious and detailed assessment of the merit of any one of them by a non-specialist reviewer is therefore clearly impossible. But it can at least be said that, the number of people who would want detailed accounts however good, of such diverse subjects between one pair of covers must be very small. Would it perhaps be possible to carry still further the grouping of papers on related topics into one volume? Or perhaps the ' separates ' could be published in limp cloth covers and the bound volume abandoned? Many would welcome this, and even libraries might tolerate it.

N. Thompson.

BOOK REVIEWS

The Scattering and Diffraction of Waves. By R. W. P. KING and TAI TSUN WU. Harvard Monographs in Applied Science, No. 7. (London : Oxford University Press, 1959.) [Pp. 218.] 32*s*. 6*d*.

Writers in the series *Harvard Monographs in Applied Science* are faced with something of a dilemma since it is the publishers' intention to provide, in their own words, " a medium for publishing the results of University research to a wider audience than would be reached by individual professional journals ". What audience should the authors aim at ? Should they attempt to present their work in terms of basic principles of fairly general interest, or should they collate detailed results for workers who may wish to use them ? In the above book in this series the authors have evidently (though not explicitly) decided to take for granted most of the basic principles employed in their work and concentrate on a fairly detailed analysis of their results on the scattering of electromagnetic waves by complex obstacles. Consequently the book is likely to be of most interest to physicists and engineers actively working on problems of this nature.

The authors' main interest lies in the interpretation of the results of actual measurements, and their correlation with various theoretical approaches to the problem of scattering on complex obstacles, and to this end they present a discussion of both theoretical and experimental techniques : two fairly extensive and two shorter chapters on the theory are followed by two chapters discussing experimental methods. Unfortunately the book seems rather uneven in character, perhaps as a result of its confinement to subjects related to the work of a single laboratory, so that while some topics, notably an extension of Fock's basic work on surface currents, are elaborated in a fair amount of descriptive detail, other topics are treated in a rather cursory manner, and one feels that the selection of subject matter tends to lack a guiding principle. There is some mention of the application of the results to related work in acoustics, though this is in fact confined to a statement of the acoustic analogue in specific cases. S. DONIACH.

Beiträge Zur Entwicklung der Physik Festgabe zur 70. *Geburtstag von Professor Paul Scherrer*. (Helvetica Physica Acta, Supp. V.) (Birkhaüser Verlag, 1960.) [Pp. 255.]

Many continental journals have the pleasant custom of devoting occasional issues to a collection of papers dedicated and presented to some outstanding contemporary on the occasion of his 60th birthday or on some other suitably chosen occasion. Most of the papers in such a collection will have been specially written for it. The present volume is an example of this, being a special supplement to the Helvetica Physica Acta dedicated to the doyen of Swiss physics, Professor P. Scherrer, on the occasion of his seventieth birthday. In the world outside Switzerland Scherrer is probably best known for the Debye-Scherrer powder method in X-ray physics—developed when he was a student in Debye's department in Göttingen. When Debye went to Zürich in 1920 Scherrer went with him and he has been a professor at the Eidgenössische Technische Hochschule (ETH) since then. It is mainly under his guidance that the Physical Institute of the ETH at Zürich has developed to one of the outstanding departments in the world. Scherrer is renowned not only for his catholic interest in physics, but also for his brilliant lecturing—always accompanied with carefully prepared lecture-room demonstrations. The volume under review shews clearly these two facets of Scherrer's activities. The first contributions give a picture of Scherrer as head of the department and lecturer, and tell us of the immense care taken in the preparations of the lecture-room demonstrations. After that the many different aspects of physics investigated at the ETH are one by one discussed in authoritative review articles. Among the subjects covered are : X-ray scattering, paramagnetism, ferroelectricity, neutron physics, accelerators, beta-decay, angular correlation experiments, F-centres, nuclear energy, and gas discharges. Apart from the light shed on the work and personality of an outstanding physicist this volume will also be of interest to many people because it provides a number of well-written review articles in so many different fields. D. TER HAAR.

Cryophysics. By K. Mendelssohn. (Interscience Publishers Inc., New York and London, 1960.) [Pp. 183.] $4.50.

This is an interesting book which should be read with the author's remarks in the preface very much in mind. "A reasonably short but comprehensive " introduction to low temperature physics aimed at " the last undergraduate year or the first graduate year " in which " a conscious effort has been made to curb unbalanced enthusiasm for current research ". The author is also refreshingly firm in the view that low temperature physics is a subject in its own right.

Each chapter serves as a useful introduction to a branch of low temperature physics. The first two are comparatively short and deal with methods of generating low temperatures and with thermometry. The main features of work concerned with specific heats, magnetism, and transport phenomena, follow in successive chapters. Superconductivity and the problems presented by liquid helium (both He^4 and He^3) are then dealt with rather more extensively. The last chapter contains the ' debris ', including some notes on masers and bubble chambers. There are useful appendices containing the vapour pressures of liquid hydrogen, He^4, and He^3, which of course are the basic thermometric data at low temperatures, besides values of the Debye function and Debye characteristic temperatures.

Throughout the author has used the minimum of mathematics and this book should be readily followed by science teachers and others wishing to refresh the outline of their knowledge, besides students making their first acquaintance with low temperature physics. At the end of each chapter there are useful references to review articles or more advanced books for further reading. The question of subject matter and how it is weighted in a book of this sort is very much a matter of personal choice. Thus a little more on magnetism and transport phenomena could have been desired. Nevertheless, for the student here is a useful and readable introduction to a very active field of modern physics.

D. H. Parkinson.

The Exploration of the Solar System. By Felix Godwin. (London : Chapman & Hall, 1960.) [Pp. 200.] 35*s*.

This comprehensive survey of possible developments in astronautics over the next 150 years describes in detail designs for building rocket-ships, plans for provisioning them, and the tactics and strategy of colonizing the less inhospitable planets. Many of the problems that will face the space-traveller are discussed, and the author gives some possible solutions, a number of which are his own ideas. Mr. Godwin was only nineteen years old when he wrote this book ; he is well versed in the authorities on the subject, and writes with a competence that might be envied by authors several times his age. The book is fascinating and easily readable.

B. J. Hoole.

The Neutrino. By J. S. Allen. (Princeton : University Press. London : Oxford University Press, 1960.) [Pp. 168.] 36*s*.

The neutrino is the most elusive of the elementary particles. Its existence was first postulated by Pauli in the early 1930's, to explain the apparent lack of conservation of energy observed in the process of beta-decay. According to Pauli the particle should have zero rest-mass and charge ; it is not surprising that twenty years elapsed before positive evidence was obtained of its existence.

Allen's book represents an attempt to survey the field of neutrino physics as it existed in about 1956. The material is mainly concerned with the experimental side of the subject, and contains an extensive bibliography on the early work on neutrinos.

Unfortunately, after the major portion of the book had been completed, the concept of the non-conservation of parity in processes involving the neutrino was introduced by Lee and Yang. This revolutionary idea led to many important new experiments and although Allen has attempted to include the new material in the book, a certain loss of coherence in style has resulted. Furthermore many of the experiments were not conclusive at the time the book was written.

The subject of weak interactions and neutrinos has now settled down to a more normal rate of progress, and a second edition, incorporating the work of recent years, would be most useful.

H. Muirhead.

Bericht über die Tätigkeil der Schweizerischen Studienkommission für Atomenergie von 1946 *bis* 1958, Experientia Supp. VIII. (Report on the Activities of the Swiss Commission for the Study of Atomic Energy, from 1946–1958.) Birkhäuser, Basel-Stuttgart, 1960.) [Pp. 112.]

The volume starts with a general account of the Commission and its responsibilities. This is followed by articles from seventeen laboratories and institutes describing the particular contributions made therein. Numerous photographs of experimental installations are included. The topics treated range from fundamental nuclear physics and particle accelerators to cosmic rays and medical physics by way of chemical processes, the production of heavy water, and studies of the distribution of radioactivity in Switzerland. The articles are concise in style and contain much information. Considered as a whole, the volume is well worth reading. C. A. HOGARTH.

University Physics. By F. C. CHAMPION. (London: Blackie & Son Ltd.) [Pp. 786.] 30*s*.

This book contains the revised versions of the author's five volumes—general physics, heat, light, wave motion and sound, electricity and magnetism—and is said to be intended for students taking a first-year course in physics at a University. It contains a wealth of basic information but, inevitably, some parts which seem important to the present reader receive so little attention as to be of doubtful value to the beginner. As examples one may mention alternating current theory and the physics of interference and diffraction (the experimental accounts of these topics are quite complete) while the brevity of the discussion of the whole field of atomic physics is to be regretted.

It is probable that one can devise a first-year course around a given book, whereas no one book is likely to cover fully courses which already exist. The present volume will go some way towards satisfying the general requirements but it would have been helpful had each chapter contained a brief note directing the young student towards appropriate more detailed accounts. K. J. STANDLEY.

Listening in the Dark: the Acoustic Orientation of Bats and Men. By DONALD R. GRIFFIN. (New York: Yale University Press, 1958.) [Pp. xviii + 413.] 60*s*.

Shortly after Griffin and Galambos' original experiments on echolocation by bats were published in 1940, I sent a write-up of them to a well-known country magazine, and the editor's reaction showed that he thought I was pulling his leg. His incredulity was much the same as that of the scientific world during the previous hundred and fifty years, for the principle that bats avoid obstacles by hearing was established by the experiments of Jurine and Spallanzani at the end of the eighteenth century and was put on a reasonable theoretical basis by Hartridge in 1920, but was accepted by hardly anyone.

The present book is not only an account of all that is known of echolocation by bats and the few other creatures—insects, birds, whales, rodents and men—that are known or suspected to use it, but a review of hearing in general, and above all a history of a thrilling piece of scientific discovery. It demonstrates what a big part chance plays in the increase of knowledge, and how opportunity must be seized. A young zoologist keen on bats, a professor of physics expert in ultrasonics and willing to lend his time and apparatus to a junior from another department, and the war-time development of physical apparatus for quite other purposes, were all necessary before an old (and in principle simple) problem could be solved. The book is fascinating, and (except for a few lapses such as the common confusion of multiplication and division in phrases such as 'several times smaller' and 'divided by a factor of 10') well written, but one must ask whom it is written for. The zoologist will already know most of the conclusions, which have been published before, the physicist will presumably be bored with the discussion of acoustics, and the general reader would find most of it hard going. Students, if they ever had time to read, ought to enjoy it, but they would probably be better employed reading Gibbon or Butterfield or even T. S. Eliot. Perhaps it would be best used as bedside reading for physicists; it would give them an insight into the work of other scientists, and might even show them that their skill could usefully be directed to pure science and practical problems other than destruction and mass entertainment.

W. B. YAPP.

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